

Biographical Memoirs

NATIONAL ACADEMY OF SCIENCES

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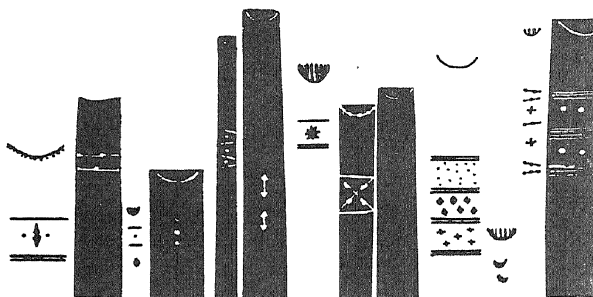
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Biographical Memoirs

NATIONAL ACADEMY OF SCIENCES

NATIONAL ACADEMY OF SCIENCES
OF THE UNITED STATES OF AMERICA

Biographical Memoirs

VOLUME XXXIII

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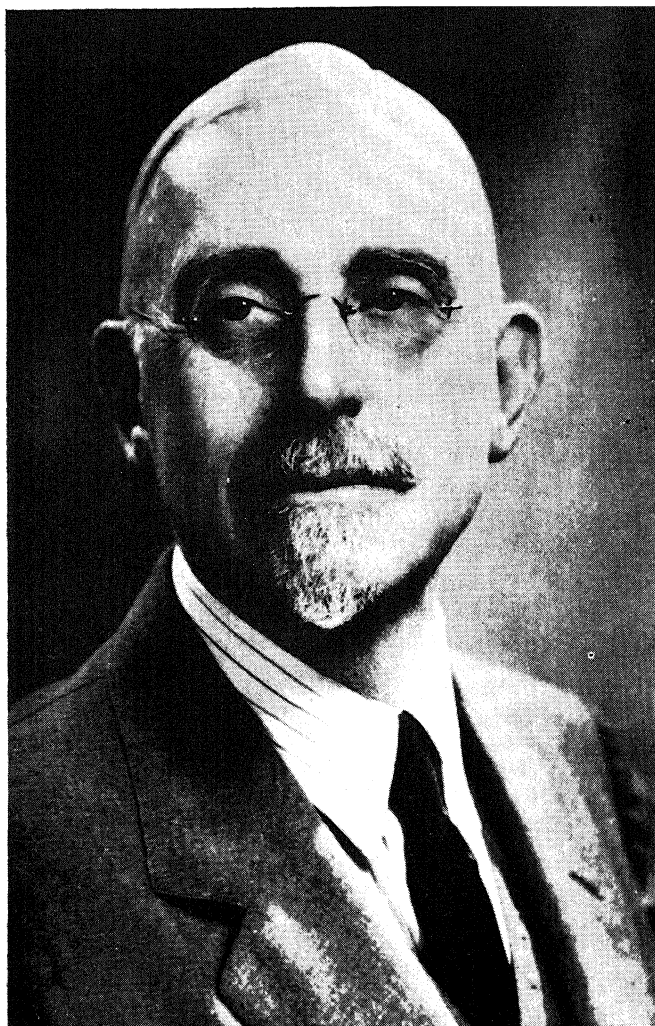
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Biographical Memoirs

VOLUME XXXIII



A. J. Blakeslee

ALBERT FRANCIS BLAKESLEE

November 9, 1874–November 16, 1954

BY EDMUND W. SINNOTT

IN THE DEATH OF Albert Francis Blakeslee on November 16, 1954, American botany lost one of its most notable leaders, a man remarkable not only for the high quality of his scientific attainments but also for the breadth of his interests and his friendly concern for the people around him. As a geneticist, he recognized the importance of heredity in determining the character of an organism, but as one concerned for many years in the cultivation of plants, he also knew the necessity of good environment. He himself was fortunate in both these things, since he came from superior human stock and was brought up in surroundings conducive to the full development of his capacities.

Blakeslee was born in Geneseo, New York, on November 9, 1874, in the home of his maternal grandfather. His father, Francis Durbin Blakeslee, was a Methodist minister and educator. Besides serving a number of churches, he had been principal of the East Greenwich (Rhode Island) Academy and of the Casenovia (New York) Seminary. His father's father was also a lifelong Methodist minister. Blakeslee's mother, Augusta Miranda Hubbard, was a remarkable woman, highly gifted and with a brilliant mind, and had a very great influence on the character of her son. She was a daughter of the Hon. Solomon Hubbard, a lawyer and judge of Geneseo, New York, who was an uncle of the author Elbert Hubbard. On both sides Blakeslee's family goes back to early New England.

Most of his boyhood was spent in East Greenwich. He loved out-

door life. Very early he showed the strong interest in natural history which distinguished him later, and he collected all sorts of things. A minister once asked him which he liked better, books or bugs. "Bugs," said Bert, "for man made books but God made bugs!" He early showed the originality and nonconformity that later were such important elements in his character.

Blakeslee was fond of the water and was a skillful sailor of small boats on Narragansett Bay. For one summer he worked as deck hand on a private yacht.

His early education was at the East Greenwich Academy, where his father was principal and his mother preceptress. From here he entered Wesleyan University, where he was college tennis champion (almost of national amateur caliber), received his letter in football, made Phi Beta Kappa and won prizes in mathematics and chemistry. After graduating *cum laude* in 1896, he taught mathematics and the sciences in Montpelier Seminary, Vermont, for two years and the same subjects for another year in the East Greenwich Academy.

Teaching was very congenial to Blakeslee and he planned to continue as a preparatory school teacher. Feeling the need for more training, however, he decided to go to Harvard for some graduate work, particularly in botany and zoology. He took a master's degree there in 1900. During this year he came under the influence of two great botanists, W. G. Farlow and Roland Thaxter. They suggested that he try his hand at research in mycology, and Thaxter offered him an assistantship. After attempting several difficult problems without much success, he undertook the classification of the Mucors and gathered a large collection of cultures of these fungi. In attempting to get zygospores for taxonomic study, he observed that they were found only under certain conditions, and this led him to the discovery that to produce zygospores in most species it was necessary to bring together two strains of opposite sexual type, (+) and (-). Such heterothallism, with its suggestion that these simple fungi reproduced by sexual fusion, was a completely new and sensational idea and has had an important influence through the years,

both in mycology and genetics. Whether these differences should be regarded as truly sexual in the strict sense may be debated, but Blakeslee so regarded them. For this work he was awarded the Bowdoin prize at Harvard. He received his Ph. D. degree in 1904. This discovery of heterothallism and the publication of his thesis established Blakeslee's reputation as an original investigator. He had found something—research—which was much more exciting than schoolteaching, and to it he devoted himself with energy and enthusiasm for the rest of his life.

During his first year at Harvard he held a teaching fellowship in botany and from 1900 to 1902 he served as instructor at Radcliffe. For two summers he was assistant at the summer school at Cold Spring Harbor, Long Island. He spent the summer of 1903 in Venezuela collecting for the Cryptogamic Herbarium of Harvard.

The Carnegie Institution recognized his ability by a grant which made it possible for him to spend two years abroad, working chiefly in the laboratory of Professor Klebs at Halle. Here he carried on his work with fungi and was able to prove that in some of the *Mucors* sex determination was made in the zygosporangium, all spores from one zygosporangium being either (+) or (—). In other forms, however, this determination occurs later, so that both types are formed in the sporangium coming from one zygosporangium.

During his first visit to Europe Blakeslee gained much more than scientific knowledge. He learned to speak German and made the most of his opportunities to meet European botanists and visit universities. For the first time he had a chance to acquaint himself with the wealth of art that Europe had to offer, and it made a great impression on him. His letters to his mother show how interested he became in it and the many museums he visited. He gathered a wealth of anecdotes and experiences, too, with which he later regaled his friends, such as the story of his temporary arrest for dashing out into the street and scraping up horse-droppings into a paper bag. He had difficulty in persuading the police that he wanted this material as a means for finding new fungi!

On his return to the United States, Blakeslee went back to Harvard for a year as instructor in botany. Positions were scarce and in 1907 he took the post of Professor of Botany and Director of the Summer School at what then was little more than a farm school, the Connecticut Agricultural College at Storrs (now the University of Connecticut). He was the only Ph. D. on the faculty at that time, but entered on the work with his usual enthusiasm, and spent there eight pleasant and profitable years. He took an active part in the life of the college and proved himself a remarkably fine teacher.

After going to Storrs he found it difficult to carry on his Mucor work, and he also felt that his research should be more in harmony with the nature of the institution. He therefore cast about for problems, but at first failed to find a suitable one. He often remarked that for several years he was almost sterile scientifically. In his course for forestry students, however, he developed keys for the identification of trees in their winter condition, and in cooperation with the horticulturalist at the Experiment Station, C. D. Jarvis, he wrote in 1911 a bulletin entitled "New England Trees in Winter," fully illustrated by photographs which the authors took at various places in New England. The bulletin was later published (1913) as a book, *Trees in Winter*, and became one of the best of its kind. Blakeslee lectured rather widely on trees and aroused much interest in their study during a time of year when they are without their distinctive foliage. In connection with this work he did the first of his genetic experiments with higher plants by making some crosses between a number of tree species, one of them resulting in what was probably the first hybrid pine produced in this country.

He continued to serve for several years as Director of the Summer School, which was widely attended by schoolteachers and others. He always had a party at the conclusion of the course, and one year thought it would be appropriate, at an agricultural college, to serve cider. In the summer, however, all he could find was some hard cider. He masked its vigorous flavor by adding copious amounts of sugar, and it was generously distributed and consumed. Nature was

not to be denied, however, and the effect of this brew upon the schoolmarms was said to have been remarkable!

All but a few of the students in the college were planning careers in agriculture or home economics. Blakeslee, therefore, felt that botany should make a contribution to the major purpose of the institution. Accordingly he conceived the idea of developing an agricultural botanical garden in which the students could see growing representatives of the important crop and horticultural plants. He obtained permission to take over a small piece of land for this purpose, and with his usual energy he got it under cultivation and soon made it a valuable adjunct to course work in botany and agriculture. At the meeting of the A. A. A. S. in 1909 he took part in a symposium before Section G with a paper on "The Botanic Garden as a Field Museum of Agriculture." He devoted much effort to expanding this garden and its usefulness during his remaining years at Storrs.

To more strictly agricultural problems he also made contributions. Observing that the fancy points by which poultry were judged at shows seemed to have little relation to their productivity, he tried to find characters that might be correlated with high egg yield. Pale yellow color of legs, beak, and vent seemed such a trait, due presumably to the withdrawal of yellow pigment from the bird's body to the egg yolks. The International Egg-laying Contest had been started at Storrs and this made data available on hundreds of hens with trap-nest records. To measure intensity of pigment Blakeslee used a color top by which varying proportions of yellow and white could be blended. He and Professor Warner of the poultry department measured the color of hundreds of birds and established a close negative correlation between the intensity of color and the time since the last egg was laid. By this means culling of unproductive birds could be done much more simply than by trap-nesting—a real contribution to agricultural practice. Blakeslee tried to do the same thing with cows, by studying the color of the "dandruff" at the end of the tail, on the assumption that this would be paler in the animals which produced more cream. This technique proved less profitable, however.

About this time Blakeslee's interest in genetics began to develop, particularly in connection with some of the plants that he was growing in the botanical garden. He began to look for mutations, and thought he had found a mutating species in *Rudbeckia hirta*, the black-eyed Susan, which he collected extensively. This proved difficult material, however, though he was able to distinguish chemically two complementary yellow-coned types which, when crossed together, produced plants with purple cones.

Much more important proved to be his introduction to the Jimson weed, *Datura stramonium*. Blakeslee had received from the United States Department of Agriculture for the Botanical Garden a package of seed of this species as an example of an economic weed. The lot happened to include both white-flowered and purple-flowered types, a difference produced by a single gene. Soon after this, seed of the smooth-capsuled type (the normal plants are spiny) was also obtained, so that two different pairs of genes were available. This made excellent material for class work in genetics, since various ratios could be produced which students might study in the garden. These *Datura* cultures were the basis of much of his later work in genetics.

Blakeslee was invited to the Carnegie Institution's Station for Experimental Evolution at Cold Spring Harbor for the year 1912-1913, on leave from the college, and while there he resumed his earlier work on sexual reactions in the Mucors. Here he attempted, in cooperation with R. A. Gortner, to identify possible differences between the proteins of the two mating types by their precipitin reactions, but the extracts injected into rabbits proved so toxic that the experiment was discontinued. The powerful toxin in the common mold *Rhizopus* became of interest to the Bureau of Chemistry in Washington, and Blakeslee cooperated with the bureau by furnishing dried mycelium in quantity.

Back at Storrs, he was able to obtain from the college authorities the appointment of an instructor in botany, who relieved him of some of the pressure of teaching and allowed him to devote more

time to research. This research soon began to turn in the direction of *Datura* genetics, foreshadowing his long study of this plant. Among the cultures of it grown for class work there appeared one plant with a more rounded capsule, differing from normal in other respects as well. It was called "Globe," and its traits were transmitted infrequently through its pollen and only through about a quarter of its ovules. It later proved to be a trisomic or $2n+1$ type, one of the forms which later proved to be so important. The chromosomal basis of these was still unknown, but evidently *Datura* was going to prove interesting genetically, and Blakeslee began to study it carefully.

In his advanced work in botany he devoted more attention to genetics and in 1914-15 gave an undergraduate course in this subject, probably the first organized course in genetics in the United States. His title was now changed to Professor of Botany and Genetics.

Early in 1915 the Carnegie Institution invited him to come to Cold Spring Harbor as resident investigator in genetics to fill the post formerly held by G. H. Shull, who had been called to Princeton. Blakeslee was uncertain whether to accept or not. The opportunity for research attracted him, but he was happy at Storrs and loved teaching. He finally told the director, C. B. Davenport, that he would come, and promised to stay two years, but said that he planned eventually to get back to teaching. This he never had the opportunity to do until he went to Smith College in his later years.

Blakeslee not only loved to teach but did it extremely well. He had the faculty of arousing the interest and intellectual curiosity of his students to a high degree. For example, one day when the class was studying protoplasmic streaming in *Elodea* he raised the question as to how fast a plastid was carried along in the stream. A stop watch was secured and the speed was timed. Did the plastid move, relative to its length, as fast as a horse, or not? How long was a horse? The class could not agree on this, and finally went outside and measured one! Now they could translate the relative speeds of streaming in plastid and quadruped. Blakeslee taught for only a few

years but there are scores of men and women in Connecticut and elsewhere who remember their work with him as one of the high spots of their college careers.

The decision to move to Cold Spring Harbor was a grave one and quite changed the character of Blakeslee's life. His new environment was not academic and he missed not only his teaching but also the pleasant associations of a small, rural community. As compensation, however, he was now in an atmosphere of research and into this work he threw himself with his accustomed energy and enthusiasm. *Datura* he brought with him, but before concentrating on it he cast about for other favorable material for genetic research. For a time he grew adzuki beans, *Portulaca*, and many other species. Clouds of war were on the horizon, and after war came his work was somewhat disrupted, for he felt that he should direct it toward more practical ends. The war was unfortunate for him in other ways, for it took into the army his brilliant assistant, B. T. Avery, a former student whom Blakeslee had brought with him from Storrs. Avery died near the end of the war and his loss was a severe one. His brother A. G. Avery became Blakeslee's assistant and served in this capacity for twenty-eight years.

With the return of "normalcy," however, Blakeslee's research became very active and now was centered chiefly on *Datura*. One new form after another began to appear in his cultures. Some were gene mutations but many were evidently different. These produced some offspring like themselves but threw many normal plants. For an outsider to recognize these forms was difficult, since most of their differences were subtle ones. It was the despair of his colleagues to see Blakeslee go down a row of plants and pick out these mutants unerringly. This he could do partly because of his acute powers of observation and partly because he was personally familiar with his material and did not leave the observing and recording to his assistants alone.

The size of the *Datura* cultures increased and in the summer as many as 70,000 plants were grown. Work was actively carried on in the winter, as well, in the six greenhouses and laboratories.

Datura justified the choice of it as a research plant. It was easy to grow. Though naturally rank, it remained small if not given too much nitrogen, and could be made to flower and fruit early in pots or in the field. It had all the vigor and vitality of a true weed. Its disadvantages were an unpleasant odor and the possession of a powerful alkaloid, stramonium, in its sap, which sometimes produced unpleasant effects. Blakeslee became attached to this coarse, weedy plant with its beautiful flowers and once rose vigorously to its defense. Edna St. Vincent Millay, in her poem "In the Grave No Flowers," had written

Here beggar-ticks, 'tis true,
Here the rank-smelling
Thorn-apple,—and who
Would plant this by his dwelling?

This aspersion on *Datura* was too much for Blakeslee to take without a protest. "I thought I would write you," he said in a letter to Miss Millay in 1947, "and tell you the answer to your question by saying that *I* would plant this by my dwelling and have done so for the last thirty years rather extensively. It turns out that this plant (*Datura stramonium*) is perhaps the very best plant with which to discover principles of heredity." He then went on to ask her about the name "thorn-apple," and said he wished that he had used it instead of Jimson weed, the name given it by soldiers sent to Jamestown, Virginia, to put down Bacon's rebellion in 1676.

The *Datura* program did not go far in the direction of mapping genes in chromosomes, as had been done with maize and *Drosophila*, but instead concentrated on the behavior and genetic effects of individual chromosomes and their parts. This began in 1920 through the fortunate collaboration between Blakeslee and a distinguished cytologist, John Belling, then a visiting investigator at the station. By use of his aceto-carmin stain and the smear technique, Belling was able to make wholesale chromosome determinations quickly. Most of the strange mutants that had been turning up

in the *Datura* cultures proved to be trisomics, plants in which one of the twelve pairs of chromosomes was represented by three instead of two. This was an interesting discovery and explained why these forms did not breed true, for at meiosis only half of the eggs would have the extra chromosome and produce trisomics. This chromosome was seldom carried by the pollen. Belling's connection with these investigations ceased in 1924, but the cytological work was ably continued by Dorothy Bergner.

Since there are twelve pairs of chromosomes in *Datura*, there should be twelve possible trisomic types. One by one these appeared and were duly named—"Globe," "Poinsettia," "Sugar-loaf," and the rest. The unexpected advent of a thirteenth trisomic, however, showed that the problem was not quite so simple. There were evidently relationships among the trisomics, for certain ones produced others rather consistently. The problem was finally cleared up by the demonstration, genetic and cytological, that in some trisomics (the *primaries*) the extra chromosome was one of the twelve usual ones, whereas in others (the *secondaries*) it was one in which one of the two halves of this chromosome had been doubled. Thus there should be two secondaries for every primary, though these were not always found. There were also *tertiary* mutants, in which the extra chromosome was made up of parts from two different chromosomes. In *compensating* types, also $2n+1$, one member of a normal pair was missing, but this loss was made up by having the two halves of this chromosome attached to two others.

Again with Belling's help it was also possible to distinguish types in which reciprocal translocation had taken place through exchange of pieces between two nonhomologous chromosomes. Since no genes are lost in such a process, a race with one or more translocations cannot be distinguished externally. In a cross between two races differing by such translocations, however, similar chromosome ends attract each other at meiosis and rings are formed containing four, six, or more chromosomes. By crossing the original Line 1A with other races of *Datura stramonium* from various parts of the world,

Blakeslee was able to distinguish a considerable number of cryptic or prime types, races which were not yet different in their genes but had undergone a certain amount of rearrangement of chromosome material, a step in the process of evolution. This was progress toward the original goal of the station at Cold Spring Harbor—"Experimental Evolution."

Among the early cultures a plant was found which was somewhat stockier than the rest and had a broader capsule. It proved fertile with itself but was nearly sterile in crosses, and Blakeslee called it "New Species." It later proved to be a tetraploid and opened up another series of problems.

Tetraploids were useful in the production of triploids through an occasional hybrid with diploid. The triploids, in turn, were a fertile source of trisomics and other chromosomal types. More exciting than the tetraploid, however, was a haploid plant that turned up among the offspring of a normal individual. It was weak and almost sterile, but undoubtedly haploid, the first to be discovered among vascular plants. Haploid plants, botanists had been taught to believe, should be gametophytes, but this was an undoubted sporophyte. Thus Blakeslee possessed a continuous polyploid series,— $1n$, $2n$, $3n$, and $4n$ —and could determine the effects, as to size and shape, which were produced by increased number of chromosome sets.

Interest in polyploidy was increased by the discovery, in 1937, of the fact that the alkaloid colchicine under certain conditions would induce a doubling of the chromosome number. Blakeslee was one of the first to recognize the importance of this discovery and he vigorously developed its applications, not only in *Datura* but in other plants.

The most important result of the colchicine technique for the *Datura* research was that it made possible, through treatment of germinating seeds, the occasional production of periclinal chimeras, tissue mixtures where the outermost one or two cell layers at the meristem had their chromosome number doubled, or sometimes even quadrupled. Once established, these layers maintained them-

selves throughout the development of the plant. Such a chimera was of no very marked significance genetically, but Blakeslee saw at once its importance in morphology, for these cell layers, since they differ in number of chromosome sets, differ also in cell size. The structures that are formed by each layer could thus be recognized histologically. In this way it was possible to determine, through a study of cell size in leaves, petals, sepals, stamens, and pistil, what contributions to the development of each had been made by the various meristematic layers. Blakeslee and his colleague, Miss Satina, were thus able to make some very interesting contributions to our knowledge of plant morphology and development. Blakeslee regarded these layers as true germ layers, comparable in a sense to those of animals, though this was disputed by some morphologists.

Early in the *Datura* work the problem arose as to why the addition of a single chromosome, in a trisomic, often made a much greater change than the addition of a whole chromosome set. The effect of a single extra chromosome was evidently to emphasize the contribution of the genes it contained, and thus tip the balance in their direction. Adding a whole chromosome set did not have this effect, and Blakeslee recognized the importance of this concept of genic balance. The diploid should thus, in a sense, be the average of the twelve trisomic mutants.

To establish this, it was necessary to use characters which could be expressed quantitatively. For this purpose, certain structural traits proved of value. The present writer was fortunate enough to be able to collaborate in this problem. It was possible to get a fair measure of the differences between the twelve trisomics in a considerable number of anatomical characters, and the diploid turned out to be approximately the average of the twelve in most characters. Some interesting facts as to the genetic determination of anatomical traits also came from this study.

The inability to obtain seed from certain crosses, even when the pollen was apparently good and would germinate on the style, was often a block to genetic studies, and this turned Blakeslee's attention

to problems of pollen tube growth. Here he enlisted the collaboration of J. T. Buchholz, who for several summers carried on a fruitful study of it. By the use of a differential stain which he developed he was able to distinguish microscopically the pollen tubes from the stylar tissue in which they were growing, and he often found that tubes from certain sorts of pollen grew more slowly than the rest and thus did not reach the ovary in time to effect fertilization. To overcome this difficulty, Buchholz spliced a stigma directly to the top of the ovary, thus shortening the distance to the ovules and making possible many crosses which could not otherwise have been obtained.

Obviously it was important to get as large a supply of new mutant types as possible for study. Through collaboration with C. S. Gager, radium emanation was used for this purpose and produced some of the first mutations to be made by radiation among the higher plants. X rays were also employed successfully. Later, the Atomic Energy Commission requested him to study the effects of thermal neutrons and radiations from nuclear detonations and from a cyclotron in the production of chromosome and gene mutations by using the *Datura* material.

Blakeslee noticed that seed which had been kept a long time in storage produced more mutations than fresh seed, and this led him into some physiological problems. In the later years of the *Datura* work, at Smith College, he went still further into physiology by studying the development of early embryos in culture and their nutritional requirements.

In these various approaches to *Datura* genetics, the more familiar techniques of genetic analysis were not neglected, although they did not form the central theme of the work. There were 541 genes found, of which 81 were located in specific chromosomes, some in each of the 12. This was done not only by the usual means of linkage data, but also in other ways, as by the use of trisomic ratios, of compensating types, of pollen-abortion genes, and of data from certain prime types.

Aside from his success in research, two of Blakeslee's qualities are conspicuous in this *Datura* program. He was always on the watch for new lines of attack on the basic problem, approaching it not only from the point of view of genetics but also from those of cytology, evolution, anatomy, morphology, and physiology; and he drew into collaboration with him a group of scientists with special knowledge of their particular fields who were able to contribute to the solution of the *Datura* problems. It was characteristic of him, too, that he provided his collaborators with good support and left them free to work out their ideas, helping them in whatever way he could. In the true sense of the word the *Datura* program was a cooperative one. Its head was no dictator, but simply *primus inter pares*.

Although *Datura* held the center of his attention scientifically for most of his productive life, Blakeslee's interests were so wide that he was continually exploring other problems. Notable among these was the genetics of taste. In 1917 he had noticed that a pink-flowering *Verbena* plant which had a pleasant odor to him was without odor to his assistant, and that a red one which was odorless to him was fragrant to his assistant. He began to watch for differences in olfactory acuteness and then in taste. In 1931 he discovered that there were sharp differences among people in their ability to taste various chemical substances, notably phenyl-thio-carbamide (PTC). To some this was tasteless but to many others very bitter, and individual sensory thresholds to concentrations of it were markedly different. Furthermore, the ability to taste it seemed to be inherited. This interested Blakeslee intensely, not only from a genetic viewpoint but also because of its bearing on fundamental human differences in reaction to the outside world. At several meetings of scientific associations he set up a little booth where hundreds of people were tested for their PTC reactions, and gathered some very interesting data.

The basis of sex was another field of interest. His earlier work involved the demonstration of what he regarded as sexual differences in molds, and he was always anxious to find a chemical basis for

this. When he learned of the Manoilov reaction, he at once applied it to his *Mucors* and found that the (+) and (—) strains reacted differently to it. He then tried it out on certain dioecious plants and could separate the staminate from the pistillate by its means.

He was much interested in better techniques for his work and invented some himself. Buchholz's novel methods in the study of pollen tube growth delighted him. He tried tissue cultures in various problems. He and J. van Overbeek endeavored to induce parthenogenesis by injecting various chemicals into young ovaries. Later Blakeslee and his students grew *Datura* embryos in culture, and in 1944 he convened at Smith College an important conference on embryo culture.

Research did not comprise Blakeslee's entire interest. He ran his own *Datura* program smoothly and efficiently and in an atmosphere of good will. It was only natural, therefore, that on Dr. C. B. Davenport's retirement in 1934 Blakeslee should be chosen acting director of the station, and then director in 1935. This required a good deal of time and thought, but he gave it gladly and made an excellent administrator. He was interested not only in the scientific program of the laboratory but also in the welfare of the members of his staff, young and old.

In 1941 Blakeslee reached the age of retirement and left his post at Cold Spring Harbor where he had labored so successfully for twenty-six years. Still vigorous in mind and body and eager to continue his work, he cast about for an opportunity to do so. He had the good fortune, at this critical time, to be invited to serve as William Allan Neilson Research Professor of Botany at Smith College for 1942-43. The next year he was appointed visiting professor there and, thanks to support from other sources, as well, he organized the Smith College Genetics Experiment Station, bringing to Northampton two former members (Satina and Avery) of his staff at Cold Spring Harbor. He succeeded in getting funds for a new greenhouse. To set up this station and get it supported and running smoothly required no little executive ability, diplomacy, and persistence. He

had to secure financial support from many societies, foundations, and private donors, and was very successful in doing this. To develop a competent staff was another problem. That these jobs were well done is testified by the fact that Blakeslee and his group had twelve very productive years at Smith. Here *Datura* had its second blooming. The list of papers turned out from the new laboratory is imposing, both as to their quality and the extent of the fields covered. Blakeslee not only continued his earlier studies in *Datura* genetics, but expanded it to include work on the physiology and embryology of this plant. In it J. Rappaport and afterward J. Rietsma cooperated.

Quite outside his research interests, Blakeslee was a good scientific citizen and devoted his energy, good sense, and wide acquaintance to the solution of a number of administrative problems in the biological profession. In 1935 when the *American Journal of Botany* was having financial difficulties and was unable to publish all the worthy material sent to it without a delay of two years, he was made chairman of a committee to study the situation. To this he devoted much time, and his committee's report was the basis for a reorganization of the publication policies of the Society and the *Journal* which resulted in a great reduction in the costs of publication.

Blakeslee was president of the Trustees of *Biological Abstracts* when that journal was going through similar financial difficulties, and his initiative and good judgment did much to avert a catastrophe to this useful publication. He brought the experience he had gained with these two publications to the assistance of others.

He was the original instigator of the movement which led to the establishment of the National Science Fund. He was a member of the Board of Managers of the New York Botanical Garden. He was president of various scientific societies, among them the American Association for the Advancement of Science. These offices he regarded not simply as honorific assignments to him but as opportunities to advance the interests of the organizations. His presence at the often somewhat formal meetings of their officers was like a breath of fresh air, and he was instrumental in inaugurating many

new ideas and policies. He was made a member of many foreign scientific societies and received honorary degrees from universities of four continents (see list of honors).

Blakeslee's interests were not confined to existing scientific organizations. In one case, at least, he started a new one. Not long after coming to Smith College in 1942 he suggested that the faculty and students at Smith, Mt. Holyoke, Amherst, and the University of Massachusetts—institutions located near each other—who were interested in genetics and related fields should get together from time to time for discussion of problems in these subjects and for the presentation of papers. The Four-College Genetics Conference was the result, an organization which has done much to stimulate interest in genetics in these institutions.

There was probably no botanist in the country who had as wide an acquaintance among the botanical profession as Blakeslee had. He was usually present at the meetings of the many scientific societies to which he belonged. At these gatherings he made it a point to meet scientists whom he did not know. He exchanged ideas with them and kept in touch with developments not only in botany but in other sciences, as well. Blakeslee was also widely known among European botanists, for he always attended the International Botanical Congresses and he held important offices in them. At the last one, in Paris in 1954, he could not be present because of ill health, though until the last minute he had planned to attend.

In 1924 the Carnegie Institution sent him as its delegate to the Pan-American Scientific Congress at Lima, Peru, and in 1947-48 he was delegate of the American Association for the Advancement of Science to the Indian Scientific Congress. In 1929 he was elected to membership in the National Academy of Sciences. In 1931-33 he was a member of the Division of Biology and Agriculture of the National Research Council.

Harvard recognized his qualities and made him a lecturer there in 1948-49, where he gave a course in genetics. He was also a mem-

ber of the Visiting Committee for Biology and the Bussey Institution in 1952.

Beyond this work in his professional organizations, Blakeslee was broadly interested in human affairs generally, and especially in education. At Storrs he wrote a paper entitled "Corn and Men," in which he showed that human beings vary genetically just as corn does, and that they are also susceptible to environmental influences. In "Corn and Education," published later, he amplified this idea. Later he had the opportunity to show in a practical way his interest in education, for he was long the chairman of the trustees of the Cold Spring Harbor School District.

He made many suggestions for the consideration of educators and, though these were not all accepted, they stimulated thought. One of them was to establish a school in New York City in which all the pupils should be identical twins. Investigation showed him that there were about 150 pairs of such twins in the New York public school system. By the use of identical twins, genetic differences would be eliminated. He proposed that one of each pair be taught by the new educational techniques and the other by traditional methods, as a control. The idea still seems to be sound!

Another suggestion was that the Carnegie Foundation award pensions to deserving individual teachers regardless of their institution, and not simply to members of the faculties of a selected list of universities. Still another suggestion, to the universities this time, was to use their honorary degrees more productively. He believed that such degrees should be given more often than they now are to young and active scholars who would gain encouragement and recognition thereby. He also suggested that they be awarded to promising men in academic life who did not have the doctorate and who thus were denied preferment.

Particularly after coming to Smith College, his thoughts turned to matters of undergraduate education. He delighted in having the opportunity of associating with such students again and took a lively interest in their work. His old teaching skills came back. In a very

readable paper, "Teachers Talk too Much," he described a chapel talk he gave at Smith on the function of a college education. At its conclusion he distributed slips of paper impregnated with PTC and asked each student to taste one. This created a mild sensation, for the young ladies found that a substance violently bitter to some had no taste at all to others. This did more to convince the girls of the existence of genetic variability in the human species than any amount of talking. Few students—or faculty members—remembered long what he had said in his talk, but they never forgot the taste test.

This incident emphasizes Blakeslee's ability to dramatize his point, as a good teacher should. He was a wonderful showman and even his scientific papers were often vividly illustrated. To emphasize the human diversity in ability to taste and smell, he arranged a series of tests at the biologists' dinner at the summer meeting of the AAAS at Berkeley in 1935. Here the guests recorded their reactions to benzoate of soda, PTC of two concentrations, mannose, artichokes followed by water, and to the odors of several kinds of flowers.

In his later years he was much concerned with the intellectual life of undergraduates. He was chosen president of both the Sigma Xi and the Phi Beta Kappa chapters at Smith, and it was characteristic of the man that at the end of his term in each office he presented to the members a list of suggestions—thirty in the case of Sigma Xi!—which he felt it would be beneficial to adopt.

Not only the problems of students received his attention. From the days when he served as chairman of the social committee at the Connecticut Agricultural College through his years at Cold Spring Harbor, and especially during his service at Smith, human relations interested him. He was elected president of the Smith Faculty Club and made it a much more active center of faculty life. So successful was it that the trustees of the college voted to underwrite some of its expenses. One of his friends in industry became interested in the club and donated \$10,000 to redecorate the clubhouse and install labor-saving devices in the kitchen. During his term as president he

gave much thought to the way in which the four neighboring colleges handled the problems of human relations among their faculties and staffs, and in 1948 he invited representatives from these institutions to a Four-College Human Relations Conference at the Smith faculty club to discuss this matter.

Blakeslee was also concerned with the relations between the college and the community, often a thorny problem. As president of the Northampton Leonard Club (a town-and-gown club) he made it a more active organization and helped meet this problem.

In almost everything he did he introduced an element of good humor. This shows particularly, perhaps, in one of the last of his efforts towards improving human relations. He was impressed, as his birthdays accumulated, with the desirability of bringing together the older and retired members of the faculty and staff, for their own enjoyment and the elevation of their morale. Consequently he founded the OBND Club (Out But Not Down)! These oldsters foregathered for two dinners a year and a monthly luncheon at the faculty club.

These activities all emphasize Blakeslee's intense and friendly interest in people. In a letter he wrote to a member of OBND only ten days before his death he remarked that "being one of the begatees of clergy (children of ministers) I acquired by inheritance and environment a somewhat sensitive conscience which I have not always succeeded in keeping under control." During his whole life, this conscience was always at his elbow.

His friendly spirit and concern for the welfare of other people was evident all through his scientific career. What made the *Datura* program at Cold Spring Harbor so successful was not only Blakeslee's scientific insight, energy, and imagination but the spirit of the group who participated in it, from gardeners to professors. Every summer there gathered for work in laboratory, greenhouse, and field a considerable number of men and women from other institutions. Most of them were young, and he delighted in working with them. It was unfortunate that through these years he could have no graduate students, but these young people in a sense were his graduate stu-

dents. Many of them went on to careers in genetics and they owed not a little of their success to the stimulus that he provided.

With the older men and women, his professional colleagues, his relations were also friendly and stimulating. He gave them the best facilities available for their work and enjoyed the give-and-take of exchanging ideas with them. He was generous with everyone in the allocation of credit. Belling, who aided the *Datura* work so fruitfully, was given full recognition for his contributions, and the same is true for the others. It is significant that in most of his papers his name did not appear alone, but that he shared the authorship with others.

Blakeslee's contacts with his colleagues extended far beyond the field and laboratory. The hospitality of his home was one of the delightful aspects of working with him. No description of his life and service to his fellows can leave out the very vital share in all of it contributed by his charming wife. Margaret Dickson Bridges was a daughter of the Rev. William James Bridges of Binghamton, New York, and was a friend of Blakeslee's sister, through whom she met him. She was a graduate of Smith College and was later trained for the profession of nursing. She and Blakeslee were married on June 26, 1919. Both were past their first youth at the time, but it proved to be an ideal marriage. The Blakeslees had a large house not far from the laboratory and entertained in it with delightful charm and informality. During the summer their home always sheltered several of the visiting *Datura* workers, and sometimes their families, as well. Mrs. Blakeslee's interest in these friends of her husband was no less than his own, and she brought to their entertainment the charm of her personality. When Blakeslee became director of the station his social responsibilities were increased, and here his wife was of inestimable service to him. She also accompanied him on his trips abroad and to many of the scientific meetings in this country, and was a helpmeet to him in every sense of the word. A friend once said that Blakeslee had two great loves—Margaret and *Datura*, and in that order. Her sudden death in 1947 left him desolate and he never fully recovered from the shock of it.

The social life of the Cold Spring Harbor laboratory in the summer, centering in the Blakeslees, involved more than their home. There were beach parties and picnics which those fortunate enough to share will never forget. Blakeslee's songs, recitations, and impromptu speeches were a delight, and he was a catalyzer of fun and good times.

No story of the Blakeslees would be complete without mention of Prince, their little dachshund. This remarkable dog was with them for years and shared much of their lives. He was extremely intelligent and Blakeslee taught him a host of tricks. He would dance around the dinner table on his hind legs for a tidbit. When his master whistled, "Where did you get that hat," he fetched the hat. Not till he heard reveille whistled in the morning would he come upstairs, nor would he cross the road until the signal came. For miles he rode on the fender of his master's car, and he could distinguish the sound of this particular car from that of all the others, and would rouse himself and go to the door when it entered the station yard. To the pleasure of life in the Blakeslee circle, Prince contributed his not inconsiderable share.

Blakeslee was a many-sided person. His versatility was great and his energy and persistence boundless. He was no narrow specialist but combined a devotion to science with a warm appreciation for the humanities. He loved art sincerely and used to lecture on it at Storrs and elsewhere. He seized every chance to visit art museums and became a connoisseur of some distinction. The beauties of nature always appealed to him. Music moved him, and in his last days, sick and alone, he consoled himself by playing records of the classics that he knew so well. He loved children and had a way with them. A born nurse, he almost made medicine his career. His friends remember countless instances of his thoughtfulness and consideration for others, though he would often pass these off with a smile or jest. He was a man of great reticence, and only rarely did there come to the surface the deep feelings that stirred him. In his death science has lost a skilled practitioner, but to his friends, scientists and laymen alike, the loss is a deeply personal one.

HONORS

MEMBERSHIP IN AMERICAN HONORS SOCIETIES:

American Academy of Arts and Sciences
American Philosophical Society
National Academy of Sciences

PRESIDENCIES OF AMERICAN SCIENTIFIC SOCIETIES:

American Association for the Advancement of Science, 1940
American Society of Naturalists, 1930
Botanical Society of America, 1950
Society for the Study of Development and Growth, 1946
Torrey Botanical Club, 1933

HONORARY MEMBERSHIP IN FOREIGN SCIENTIFIC SOCIETIES:

Genetics Society of Japan (Honorary Member)
Académie des Sciences, Institut de France (Foreign Member)
Linnaean Society of London (Foreign Member)
Mycological Society of Leningrad (Honorary Member)
Nederlandsche Botanische Vereeniging (Corresponding Member)
Royal Academy of Belgium (Associate Member)
Royal Danish Academy of Science (Foreign Member)
Royal Physiographical Society of Lund (Foreign Member)
Royal Swedish Academy of Science (Foreign Member)
Société de Biologie de Paris (Associate Member)
Society of Naturalists of Moscow (Honorary Member)
Société Royale de Botanique de Belgique (Associate Member)

HONORARY DEGREES:

D. Sc., University of San Marcos, Lima, Peru, 1925
D. Sc., Wesleyan University, 1931
LL. D., University of Arkansas, 1947
D. Sc., Yale University, 1947
D. Sc., University of Delhi, India, 1947
D. Sc., The Sorbonne, Paris, 1951
D. Sc., Smith College, 1952

MEDALS AND PRIZES:

A. Cressy Morrison Prize, New York Academy of Sciences, 1926
Henry de Jouvenal Prize, Palais de la Découverte, 1938
George Robert White Medal of Honor, Massachusetts Horticultural Society, 1952

KEY TO ABBREVIATIONS

- Amer. Biol. Teacher = American Biology Teacher
 Amer. Home = American Home
 Amer. Jour. Bot. = American Journal of Botany
 Amer. Jour. Physiol. = American Journal of Physiology
 Amer. Nat. = American Naturalist
 Amer. Phil. Soc. Yearb. = American Philosophical Society Yearbook
 Annal. Mycolog. = Annales Mycologici
 Annals N. Y. Acad. Sci. = Annals of the New York Academy of Sciences
 Biochem. Bull. = Biochemical Bulletin
 Biol. Bull. = Biological Bulletin
 Bot. Gaz. = Botanical Gazette
 Brooklyn Bot. Garden Mem. = Brooklyn Botanic Garden Memoirs
 Bull. Torrey Bot. Club = Bulletin of the Torrey Botanical Club
 Carnegie Inst. Wash. Publ. = Carnegie Institution of Washington Publications
 Compt. Rend. Acad. Sci. Paris = Comptes-rendu, Académie des Sciences, Paris
 Jour. Genet. = Journal of Genetics
 Jour. Hered. = Journal of Heredity
 Mem. Hort. Soc. N. Y. = Memoirs of the Horticultural Society of New York
 Mem. N. Y. Bot. Garden = Memoirs of the New York Botanical Garden
 Mycol. Centralbl. = Mycologisches Centralblatt
 Nat. Hort. Mag. = National Horticultural Magazine
 Proc. Amer. Acad. Arts Sci. = Proceedings of the American Academy of Arts and Sciences
 Proc. Amer. Phil. Soc. = Proceedings of the American Philosophical Society
 Proc. First Nat. Cancer Conf. = Proceedings of the First National Cancer Conference
 Proc. Int. Cong. Pl. Sci. = Proceedings of the International Congress of Plant Sciences
 Proc. Nat. Acad. Sci. = Proceedings of the National Academy of Sciences
 Proc. Seventh Int. Cong. Genet. = Proceedings of the Seventh International Congress of Genetics
 Proc. Sixth Int. Cong. Genet. = Proceedings of the Sixth International Congress of Genetics
 Proc. Soc. Exp. Biol. Med. = Proceedings of the Society for Experimental Biology and Medicine
 Records Genet. Soc. Amer. = Records of the Genetics Society of America
 Sci. Monthly = Scientific Monthly
 Storrs Exp. Sta. Bull. = Storrs (Conn.) Experiment Station Bulletin
 U. S. D. A. Handbook = United States Department of Agriculture Handbook
 U. S. D. A. Yearb. = United States Department of Agriculture Yearbook
 Yearb. United Hort. = Yearbook of United Horticulture
 Zeitschr. Ind. Abst. Vererb. = Zeitschrift für Inductive Abstammungs- und Vererbungslehre

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Israh Borman

ISAIAH BOWMAN

December 26, 1878–January 6, 1950

BY JOHN K. WRIGHT AND GEORGE F. CARTER

1878–1935, by John K. Wright

ISAIAH BOWMAN was born at Waterloo, Ontario, on December 26, 1878, and died in Baltimore on January 6, 1950, at the age of seventy-one. He will be remembered for his achievements in the advancement of geography, the administration of institutions of higher learning, and the shaping of governmental policies. This memoir outlines the main events of his life and comments upon his scientific work and ideas, particularly in the field of geography. It touches but lightly upon his services as an adviser to the United States Government. His influence was important enough and his personality sufficiently vivid to make it seem beyond doubt that someone will eventually write a full-length biography of him. Meanwhile, for a discerning memoir the reader is referred to Dr. Wrigley's moving tribute.¹

Of the total of Bowman's publications, about four fifths bear on geography and allied subjects. The remainder consists mainly of addresses delivered on ceremonial occasions and of reports on educational and other themes. Of the geographical four fifths nearly 95 percent had appeared before 1935, when Bowman became president of the Johns Hopkins University.

Bowman's creative intellectual powers stemmed from five personal endowments. First among them I should place an enthusiastic

¹ Gladys M. Wrigley: "Isaiah Bowman," *Geographical Review*, 41 (1951), 7–65.

imaginative curiosity. Few scholars have had a stronger feeling for poetry. Bowman saw things in sharply contrasting colors. To him there was drama no less in the clash of natural forces—in the physiographic processes of mountain building and erosion—than in the conflict of human interests and passions. At the Paris Peace Conference gods contended with Titans. The depression swept the land like a hurricane. Even in his scientific writings he made frequent use of metaphors, superlatives, and categorical assertions, assuming that no intelligent reader would take them too literally.

Bowman came of Puritan stock, and the second endowment was self-discipline, which he applied to mind no less than to body. His teachers, Mark Jefferson and William Morris Davis, helped him develop a critical habit that gave rationality to his thought and kept the torrents of his enthusiasm within useful bounds.

The third endowment was self-reliance. Open-minded enough to see that most questions have two sides, he was never inhibited from ardent advocacy of the side he preferred. Nor was he a stickler for invariable consistency in adhering to once-held ideas.

The fourth endowment, articulateness, had its roots in the three just mentioned and in his evangelical inheritance. Bowman was a fluent speaker and writer. Words came easily, sometimes too easily, to his aid, and they were full of force. Helped by a retentive memory, this "gift of gab" was ingrained, but his college teaching converted it into a gift of expression that could be eloquent. Unfortunately, some of his writings dating from his busiest years betray signs of haste, but even the least well-organized sparkle with quotable phrases and sentences.

These endowments of character and mind were made effective by a fifth: vitality. Bowman's physical constitution, if not remarkably robust, was good, and his ability to work long and hard was amazing. Though his face was often drawn with fatigue, the fires of his nature drove him on relentlessly.

The following account deals separately with four periods into

which Bowman's life may be divided: (1) twenty-six and a half years of preparation, to 1905; (2) ten years of teaching at Yale (1905-1915); (3) twenty as Director of the American Geographical Society of New York (1915-1935); and (4) fourteen and a half as President (1935-1948) and President Emeritus (1948-1950) of the Johns Hopkins University, Baltimore.

PREPARATION, 1878-1905

Isaiah's father was a farmer, but there were also teachers and preachers among his forebears and kinsmen. Early in 1879 the family moved to a farm in St. Clair County, Michigan, where at a tender age the boy developed a love for reading and, as he put it many years later, found Captain Cook's *Voyages* "thrilling and exciting." Bowman often spoke of being thrilled or excited by an experience, a book, an idea, and his excitement was infectious.

At eighteen he began four years of country school teaching, with attendance at summer sessions, and toward the end of this time decided "to study geography professionally"—a bold decision in an era when geography was almost universally regarded as little more than an elementary school subject. Bowman's professional preparation took five years (or nine, until he received his doctorate). The first year he spent at the excellent Ferris Institute at Big Rapids, Michigan, and the next four alternately at the State Normal College at Ypsilanti and at Harvard University. In 1905 Harvard awarded him the degree of B.Sc. and in 1909 Yale made him a Ph.D. His doctoral dissertation was on "The Physiography of the Central Andes."

Jefferson was the attraction at Ypsilanti, Davis at Harvard. Though well grounded in meteorology and physiography, Mark Jefferson was interested primarily in the geography of mankind, whereas William Morris Davis was concerned first and foremost with physical phenomena. Of Davis, Bowman wrote: "His incessantly inquiring mind, schooled in astronomy, geology, meteorology, and physi-

ography, eventually drove toward a single goal, the analysis of land forms," and since it was already taking this "set" when Bowman was at Harvard, the chief benefit that the younger man derived from Davis came in the form of rigorous training in logical thought and accurate expression. Jefferson did more to widen Bowman's horizons and encourage his spirit of far-roaming search.

Two currents that sometimes ran separately, but were more often interwoven, appear in Bowman's work. One was of concern with the physical, the other with the human, aspects of geography, and each was due in part to the influence of Davis and of Jefferson, respectively. Until about the beginning of the First World War the "physical" current was on the whole the stronger, and certain of Bowman's writings up to that time read almost as though Davis himself were the author. In the geographical study of man and in seeking to apply the findings of physiographic and climatological research directly to the solution of practical problems Bowman made more distinctive contributions. Also, in several respects he differed more from his two masters than they did from each other. Davis and Jefferson were teachers to the core. Neither would have given up teaching in mid-career, as Bowman did, and neither had the executive ambition and ability of Bowman, in whom the scholar and the man of action were combined in about equal measure. The scholar in Bowman gave intellectual weight to his action; the urge to action lent a realistic quality to his scholarship.

THE YALE PERIOD, 1905-1915

As instructor (1905-1909) and assistant professor (1909-1915) in the Division of Geology at Yale, Bowman was a stimulating teacher—among other subjects, of physical, political, and commercial geography, and of the regional geography of North and South America—and his courses were no "snaps." During his early years at Yale he put in several seasons of field work on water-supply problems for the United States and Indiana State geological surveys. One of his

earliest publications dealt with the pollution by oil wastes of the water supply of a small Indiana city, a problem for which he offered a solution in the light of a field study of the local drainage pattern. Other works on allied topics followed, including a comprehensive treatise on well-drilling methods in the United States. In 1907, 1911, and 1913 he participated in expeditions into the regions of South America that include southern Peru, northern Chile, northwestern Argentina, and the highlands and adjacent lowlands of Bolivia. Bowman himself organized and led the first and last of these, under the sponsorship, respectively, of Yale University and the American Geographical Society. On the second, the Yale Peruvian Expedition of 1911, he served under Hiram Bingham as geographer, geologist, and surveyor of the party. With a fire of energy and enthusiasm Bowman studied widely diverse phases of the geography of the areas visited and recorded the results in a spate of articles published while he was at Yale and subsequently in two books: *The Andes of Southern Peru* (1916) and *Desert Trails of Atacama* (1924). These travels took him both to relatively well-known places and into the wilds, where he had the explorer's full share of adventure and hardship.

Probably the clearest, simplest, least pretentious, and, therefore, most readable English that Bowman ever wrote describes what he saw and felt in South America. One example, from his account of the canyon of the Urubamba, must suffice:

"It is in such quiet stretches that one also finds the vast colonies of water skippers. They dance continuously in the sun with an incessant darting motion from right to left and back again. Occasionally one dances about in circles, then suddenly darts through the entire mass, though without colliding against his equally erratic neighbors. An up-and-down motion still further complicates the effect. It is positively bewildering to look intently at the whirling, darting multitude and try to follow their complicated motions. Every slight breath of wind brings a shock to the organization of the dance. For though they dance only in the sun, their favorite places

are the sunny spots in the shade near the bank as beneath an overhanging tree. When the wind shakes the foliage the mottled pattern of shade and sunlight is confused, the dance slows down, and the dancers become bewildered. In a storm they seek shelter in the jungle. The hot, quiet, sunlit days bring out literally millions of these tiny creatures."

"Physiographic interpretations," wrote Bowman in *The Andes*, "serve the double purpose of supplying a part of the geologic record while at the same time forming a basis for the scientific study of the surface distribution of living forms," and he used them to both ends in his South American work. When considering such strictly physiographic matters as the uplift of the Central Andes, the origin of Andean cirques, and the asymmetrical crest lines and valley profiles of the mountainous areas he hewed to the Davisian line, but departed widely from it in his perhaps more congenial investigations of the regional relationships of climate and physiography to the Andean peoples. He had already reached the conviction, as he was to put it many years later, that "the regional synthesis of life is the geographer's first concern" and "the heart of the subject is man in relation to the earth."

"The principles of geographic science," he noted in a paper on his expedition of 1913, "rest upon the theory that man is to an important degree the product of the earth," and in the preface to *The Andes* (1916) we read: "The strong climatic and topographic contrasts and the varied human life which the region contains are of geographic interest chiefly because they present so many and such clear cases of environmental control within short distances." Thus, at this period Bowman was not wholly immune to the doctrine of geographical determinism then prevalent in the minds of American geographers, although he took care to caution his readers against overemphasis upon and oversimplification of the direct influences of the geographical environment upon human affairs. Theodore Roosevelt, reviewing *The Andes*, commented approvingly on Bowman's refusal to follow some of his colleagues, who "proceed to ex-

plain all the immense complexus of the forces of social causation as simply due to geographical causes . . . ,” and after the First World War Bowman explicitly repudiated geographical determinism in no uncertain terms.

No less instrumental than his South American travels in adding to Bowman’s scholarly reputation during his ten years at Yale were the many hours of library research devoted to the preparation of his first magnum opus, a 759-page volume entitled *Forest Physiography: Physiography of the United States and Principles of Soils in Relation to Forestry*. It appeared in 1911, as an outgrowth of a course given in the Yale Forestry School, and was intended in the first instance for students of forestry. The title, however, is misleading, since nearly six sevenths of the text consist of a description, region by region, of the land forms of the United States—a description which has proved to be of enduring value both to foresters and to geographers and to many others, besides. Although several attempts had previously been made to divide the country into physiographic regions, not until after 1900 did enough information become available through detailed local geological and physiographic studies and through topographic mapping to make possible as systematic and comprehensive a work as Bowman’s.

The studies of Andean population groups in their regional settings and the regional chapters in *Forest Physiography* gave expression to the “regional concept,” in the modern development of which Bowman was a pioneer in this country. When answering a questionnaire addressed to American geographers in 1907, he alone specified “regional geography” as the branch of the subject that interested him the most.

In 1909 Bowman was married to Cora Goldthwait, whose poise, good judgment, and literary sensibility were to be of immeasurable help to him through the rest of his life. Also during the Yale period, as one of Professor Davis’s right-hand men in the leading of the American Geographical Society’s Transcontinental Excursion across the United States (1912), he made lifelong friends with many of the

American and European geographers who went on that memorable tour.

THE NEW YORK PERIOD, 1915-1935

On July 1, 1915, Bowman began work as the first director of the American Geographical Society of New York. Founded 1850-1852, this was the oldest geographical society in the country. Between 1904 and 1915 it had blossomed under the guidance and with the financial aid of Archer Milton Huntington (1870-1955), who recognized the potentialities latent in its library and map collection, its scholarly tradition, and the generally high quality of its Council; but there was one serious weakness, the lack of efficient organization and administration, and Mr. Huntington probably first suggested Dr. Bowman's appointment as a remedy.

While at Yale Bowman made his contributions to geography almost wholly through the books and papers that he wrote and the classes that he taught. While at the Society he was no less productive in his own research and writing, but the equivalent of time and energy devoted at Yale to teaching was now turned to the initiation and direction of programs that were actually carried out by the Society's staff and others.

His three major achievements here were: (1) to improve the scope and quality of the Society's periodical by replacing the old *Bulletin of the American Geographical Society* with a new and better journal, *The Geographical Review*; (2) to launch the publication of several series of monographic books and brochures; and (3) to initiate work on the so-called Millionth Map of Hispanic America. In order to defray the additional expenditures for the *Geographical Review* Bowman brought about an increase in the Society's income through ingenious and successful membership drives. Donations, most of which he secured from Mr. Huntington and another counselor of the Society, Mr. James B. Ford, made the other two programs possible, at a cost of nearly a million dollars. In the period 1916-1940

the Society published some sixty monographic works as a result of arrangements that Bowman made as Director. About half of these have more than 400 pages each, and all but three or four are original works first published by the Society.

The "Millionth Map" covers in 107 sheets the whole of the American continents south of the United States and also the West Indies and is, essentially if not officially, part of the "International Map of the World on the Scale of 1:1,000,000." Bowman conceived the idea for it when he called at the Society before his expedition of 1913 to look over the maps of the regions that he planned to visit, only to find them unsatisfactory. The work began in 1920 and for twenty-five years kept busy a group of seven or eight compilers and draftsmen. The sheets have served officials of governments and corporations in the planning and conduct of numerous enterprises, and diplomats in the settlement of boundary disputes. From them countless maps in magazines, scientific works, textbooks, and travel books have been copied or reproduced, but their most genuinely appreciative users have no doubt been the engineers, army officers, scientists, explorers, tourists, and others who have taken them into the field. Appropriate, therefore, was the award (1946) of the Society's David Livingstone Centenary Medal to Dr. Bowman "for outstanding contributions to the geography of the Southern Hemisphere."

Soon after the United States entered the First World War, Dr. Bowman placed the facilities of the American Geographical Society at the Government's disposal, and, as a consequence, the Society's building became the headquarters of the "Inquiry," a group of some 150 geographers, historians, economists, statisticians, and experts in government and international law which Colonel House assembled at President Wilson's request to gather and organize information for the coming peace conference. When Wilson and the American delegation sailed for France in December, 1918, they took with them the leading members of the "Inquiry" staff and many maps and books from the Society's collections. Bowman played an influential part in the work of the "Inquiry" and more especially in Paris dur-

ing the following two years, serving as adviser on geographical matters to the American Commission and in an executive capacity within the delegation. This experience brought him a wide circle of acquaintances among scholars, statesmen, and men of affairs, and gave him the inspiration and background for the writing, in a surprisingly short time, of a book that was to appear in several editions both in English and in translations. This was *The New World: Problems in Political Geography* (1921; new editions, 1924, 1928), a survey of the politico-geographical situation throughout the world at the time. The improvement of international relations through the education of politicians and other leaders thenceforth became one of Bowman's ardent interests—a cause to which he devoted many an article and address, to the service of which he came to consider the Society's Millionth Map a great contribution, and to which he gave himself unsparingly during and after the Second World War.

In the thirties another national emergency, the depression, again saw Bowman hard at work for the Government. During the last years of his New York period he spent about half his time in Washington, serving as chairman of the National Research Council and as vice chairman of the Science Advisory Board. He reorganized and invigorated the work of the Research Council and helped establish the Science Advisory Board, which President Roosevelt created in July, 1933, upon Bowman's suggestion to advise the various departments of the Government in questions of science. Bowman had first met F. D. R. at the Peace Conference and had become better acquainted with him while Roosevelt was a Councilor of the American Geographical Society (1921-1933).

That a goodly proportion of the Society's publications dating from Bowman's directorship bear on either Latin America, the polar regions, or pioneer settlement is largely due to Bowman's personal interest in those subjects. He himself wrote two of the thirteen books on Latin American topics, and the other eleven were regarded as a part of a Hispanic American Research Program for which the Millionth Map was to provide a foundation. Although Bowman never

visited the polar regions, he was on friendly terms with many of the world's leading polar explorers—notably Stefansson, Byrd, Wilkins, Ellsworth, Mawson, and Bartlett—and all these as well as lesser lights sought and benefited by his advice. For the guidance of polar exploration along scientific channels he brought about the publication by the Society of two useful and comprehensive books (1928).

Bowman made his most original contribution to science through the development and execution of a comprehensive program for the study of pioneer settlement. In 1925 he presented the plan to the National Research Council, which endorsed it. Supported by the donation of \$100,000 from the Social Science Research Council and certain councilors of the American Geographical Society, the program bore fruit in four volumes which the Society published and in a collaborative investigation of pioneer problems in Canada that yielded eight volumes.

During the period 1925-1937 Bowman wrote a dozen or more papers and addresses on pioneer problems, as well as *The Pioneer Fringe* (1931), the first of the Society's books on the subject. A number of passages in these works are revealing. "Strong and hopeful and confident, willing to buy their dreams with hard labor . . ."; such are the successful pioneers. "Explorers of more than material realms . . . capable of magnificent moments of decision," such are their leaders. Thus Bowman unconsciously characterized himself. Had he been a poet he would have made an allegory of pioneering. On the pioneer fringe men are engaged upon a mighty "creative experiment." So also, "there is always a fighting border to human experience," and "if the border appeals to a man it is because there is a border in his own mind." Indeed, "the greatest single fact about humanity—despite its fears and hesitations—is its willingness to advance beyond the borders of experience. . . . There have been pioneers in every generation to question and to venture," and Bowman visualized himself as one of them. "Science is like the pioneer

in making things happen instead of waiting for them to happen. . . . every scientific truth goes pioneering."

He advocated and outlined "a science of settlement," but it was a "science" only to the extent that any subject may be so regarded when studied scientifically. Though Bowman presented an unimpeachable case for the application of scientific knowledge and techniques to the study of pioneer problems, he developed no distinctive and systematic principles for the guidance of such study, as Darwin did for biology or Davis for geomorphology.

While director of the Society, Bowman twice broke away from his desk for a few weeks of field work of the kind that he had done so well in South America. His love of arid lands and pioneers took him westward in the summer of 1930 to Montana, Oregon, and the Great Plains of Kansas and Nebraska, and in 1932 to the pioneer fringe northwest of Edmonton. The first journey gave him the data for "Jordan Country," a classic case study of an American pioneer community (in Montana). He also made four trips to Europe to attend International Geographical Congresses. At the Paris Congress (1931) he was elected president of the International Geographical Union, under whose auspices the Congresses have been held since 1928, and in this capacity he presided at the Warsaw Congress in 1934. As president of the Association of American Geographers, he read an address on "Planning in Pioneer Settlement" at the annual meeting at Ypsilanti in 1931, where he also bestowed a gold medal of the American Geographical Society upon his old teacher, Mark Jefferson. He was closely associated with both the National Research Council and the Social Science Research Council, not only in connection with the pioneer-belts studies, as we have seen, but in the advancement of many other enterprises. Dr. Bowman was elected to the National Academy of Sciences in 1930.

In 1930 he set forth his views concerning the scope and nature of geography at a conference of the S.S.R.C. and later elaborated and illustrated them in a volume, *Geography in Relation to the Social Sciences* (1934), in which he developed the themes that the heart of

geography is the study of man in relation to the earth and that the regional synthesis of life is the geographer's first concern. He was one of the founders of the Council on Foreign Relations in New York, served long on its board of directors, and went to London in 1929 to represent it at a Conference for the Study of International Affairs. These were but a few of the "innumerable organizations" that "sought to avail themselves of his learning, his drive, his passion for exploring the frontiers of knowledge, his practical common sense, his ability to see the forest *and* the trees."

1935-1950, by George F. Carter

THE JOHNS HOPKINS PERIOD, 1935-1950

Bowman took over the presidency of the Johns Hopkins University in 1935. His predecessor in his final report had stressed the uniqueness of Hopkins educationally and its financial limitations. This was the time when the great depression was past its low point, but the upswing was hardly noticeable in private institutions such as Hopkins. Bowman's first presidential report stressed the need to improve the financial situation, the necessity that Hopkins should not try to do everything, but that the things that it did, it should do well. He had already grasped the Hopkins program and made it his. The University was to stress research *and* teaching. Research was not just an end in itself but was intended to keep bright the flame of scholarly enthusiasm. The most important thing in a university was the student-professor relationship. At Hopkins, with its moderate size, the relations should be close and productive of individual stimulation. Bowman looked beyond the student-faculty relations, to assess the relation of the University and its product to the community. He had a strong feeling of social responsibility. This was to appear in his writing and speaking many times in some form or another, a favorite term being "the social contract." These themes were to continue to appear throughout his presidency.

The following year his presidential report stressed the place of

the graduate school at Hopkins and the result of his thinking on this problem later appeared as *The Graduate School in American Democracy*. Subsequent annual reports show that one of his major tasks was the reorganization of departments made desirable by changes in time and men. There was a tendency to consolidate subjects that had split unduly. Zoology, botany, and plant physiology were consolidated into a unified department of biology. Expansions were made in other areas. For example, departments of geography, oceanography, and aeronautics were established. Bowman noted that his advice concerning the establishing or founding of departments of geography had been sought by at least one hundred colleges and universities.

The war imposed a load on the University, stopping some work and deflecting energies into other channels. Bowman led the University into war service, and threw himself into governmental advisory work also. Of the University's contribution he was justly proud, but he was also aware that much of this work was not the University's real business. He commented wryly at one point that he was proud of the record only in terms of survival value, and that he looked forward to a more balanced educational effort again in the future. But he expressed again the thought that knowledge does not exist in a social vacuum, that science has social implications, and that the scientist must face this. He was even-handed. He also noted that the humanist should be active and creative, and not just a critic of the works of the past.

He was expressing here his strong feeling that knowledge was in and of and for society. "A university is a social agent as well as a creator and conservator of knowledge deemed to be fundamental. It operates in a given time with the social expectation that the knowledge and training of a university will have useful applications. It is not operating in a millenium: it must be demonstrably useful to the society of today as well as to the hoped for society of tomorrow." For Hopkins he continued to stress that it was involved in a creative effort, that the relationship between quality and numbers was such

that it was desirable to remain small, and that knowledge is for social use.

Bowman's administration was marked by great energy and close contact with his faculty. He was always available. He was a member of every major board and committee and with extraordinary regularity appeared as chairman of these committees.

This is the more remarkable when his other activities and the complexity of Hopkins is considered. The University structure is unusual. There is a minimum of administrative officers. The president carries many of the burdens that elsewhere are the duty of deans. Furthermore Bowman was active as an adviser to the Government in a number of capacities. For the Department of State he served as vice chairman of the Advisory Council on Post-War Foreign Policy, adviser in the Department of State, special adviser to the Secretary on postwar problems and plans, member of the United States group at the Dumbarton Oaks Conversations on International Organization, and adviser to the United States Delegation to the United Nations Conference on International Organization at San Francisco.

Bowman was a dynamic leader. He took over a University weakened by the financial crisis. In a time when the outlook was so bleak that even the subscriptions to many learned journals had been cut by the library, Bowman shouldered the tasks of leadership, of fund raising, of infusing new spirit. Bowman was a man of courage and spirit and character. He believed in the things of the spirit and he wanted the University not only to impart knowledge but to form character, and by this he meant moral character. He steered the University through the post-depression years, through the war years, and the postwar floods of GIs. In the year between his retirement and his death, he devoted much time to the Economic Cooperation Administration and began the assembling of material for a series of studies.

Unfortunately these had not reached publishable form before his life closed. Besides these finished and unfinished works, he left locked files with instructions that they were not to be opened until

twenty-five years after his death. Just what this legacy contains is unknown. It may be rich in the detailed insights that we so often wish to have when we try to reconstruct history and understand the causes of actions. It was Bowman's custom to record important conversations immediately. He once told with gusto of a conference with Churchill in which Churchill diagramed some of his ideas. Though the meat of the conference was not divulged, Bowman made it clear that he had saved the diagram and filed it "for history." Bowman early felt that he was a man of destiny. He early began saving all of his publications. These he had bound in volumes. Those otherwise fugitive papers of a great man are therefore all preserved and are available at the Johns Hopkins Library.

KEY TO ABBREVIATIONS

- Amer. Assoc. Adv. Sci. Bull.=American Association for the Advancement of Science Bulletin
 Amer. Journ. Sci.=American Journal of Science
 Ann. Assoc. Amer. Geogr.=Annals of the Association of American Geographers
 Assoc. Amer. Coll. Bull.=Association of American Colleges Bulletin
 Bull. Amer. Geogr. Soc.=Bulletin of the American Geographical Society
 Bull. Geogr. Soc. Phila.=Bulletin of the Geographical Society of Philadelphia
 Geogr. Journ.=Geographical Journal
 Geogr. Rev.=Geographical Review
 Journ. Geogr.=Journal of Geography
 Journ. Geol.=Journal of Geology
 Proc. Acad. Pol. Sci.=Proceedings of the Academy of Political Science
 Proc. Amer. Philos. Soc.=Proceedings of the American Philosophical Society
 Proc. Nat. Acad. Sci.=Proceedings of the National Academy of Sciences
 Sci. Mo.=Scientific Monthly

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E. DeGolyer

EVERETTE LEE DEGOLYER

October 9, 1886–December 14, 1956

BY A. RODGER DENISON

OF FEW MEN can it be said that they lived at the right time—that they brought to the problems and the developments of their era precisely the right combination of intelligence, foresight, and judgment—that they brought to their industry the proper balance of pioneering, aggressiveness, and timing in order to make the maximum effective contribution to it and thus leave the world richer in treasure and in ideas for their having lived. Such a man was Everette Lee DeGolyer.

Born in 1886 in a sod house in Kansas on the outer fringe of the expanding agricultural frontier of that day, he lived to build a fine mansion in Texas and to see this new agricultural frontier become a producer of vast mineral wealth and a contributor, in a substantial way, to the fabrication and manufacture of goods.

He was attracted to the science of geology at the very beginning of its application to the art of prospecting for oil. Starting with his own simple surface examination of the clues to oil deposits, he became the leader in bringing into use the classical earth instruments, the torsion balance and the seismograph, in the underground search for petroleum. He was, more than any other man, responsible for bringing to the art of prospecting the ability to probe below the earth's surface to find clues to the secrets locked up in Mother Nature's subterranean treasure house. He never ceased to have an interest in improving and extending the use of these devices.

In spite of this preoccupation with his "first love"—the science of

the earth—he found time to become an outstanding scholar and student of the history of early Spanish exploration in the Southwest, and to collect an outstanding library on the history of science and technology. As further evidence of his diverse but well-rounded personality, he found time to serve on a great number of civic projects and organizations in his home town of Dallas—to be an officer or a director of a score or more companies, and to be the financial and spiritual “angel” for the *Saturday Review of Literature*.

He lived to be honored by the highest elective offices and to be awarded the highest decorations of the American Association of Petroleum Geologists and the American Institute of Mining and Metallurgical Engineers. He was a lecturer much sought after, filled distinguished positions at three universities, and was awarded honorary degrees by six. Nine or more United States Government agencies, commissions, advisory boards, and committees called on him for service which he generously contributed.

This combination logically made his advice much sought after in business, and he was for many years the world’s leading oil consultant. The opinion of his firm, DeGolyer and MacNaughton, on an appraisal of the worth of a property or a company was accepted as final in financial and government circles the world around. This firm was at one time or another consultant to ten or more foreign governments on subjects ranging from organizing exploration programs to the proper price for oil F.O.B. tankers in the Persian Gulf.

DeGolyer organized and directed for a time the Amerada Petroleum Corporation, the Rycade Oil Corporation, the Atlatl Corporation, and others. He inspired the organizers or provided important backing for the formation of Core Laboratories, Inc., Geophysical Service, Inc., Coronado Corporation, The Felmont Corporation, and others.

DeGolyer was a man who believed in luck. In his later years he observed that technical knowledge and skill were needed to find oil, but to be really successful you must also have luck. Said he, “If you must try to get along without one or the other, it is better to have

luck." This attitude may have been based at least in part on the fact that his earliest entry into commercial geology was a sequence of events that might well be called luck. Hired as a teamster and cook for a summer field party of the United States Geological Survey in the Bighorn Basin of Wyoming, he was given a chance to do geology because the son of an eastern politician attached to the party "flunked out." DeGolyer alone among the cook-teamsters had studied surveying. Doing geological mapping brought him to the attention of C. W. Hayes, then Chief Geologist of the U.S.G.S. When Hayes quit to join the Mexican Eagle Oil Company, Ltd., as geological consultant, he remembered young DeGolyer and hired him to work in Mexico. As geologist for the Mexican Eagle Oil Company, Ltd., he mapped the famous "Golden Lane" and advised locations for the discovery wells at the Tierra Amarillo, Los Naranjos, and Protrero del Llano fields. The Protrero del Llano #4 in the latter field was no doubt the largest and one of the most spectacular oil wells ever completed in the western hemisphere. Its initial production after flowing wild for twenty-four days was 100,000 barrels, and in its full life of eight years it produced in excess of one hundred million barrels.

As one writer reported, "DeGolyer was fascinated with luck. 'I hate to tell you,' said De, 'how many times I've made money by going against my own judgment.'" Yet with all his belief in luck De, more than any single man, brought to the art of oil prospecting a series of precise "tools" and pioneered in most cases the use and interpretation of the results. The application of these "tools" has been responsible for the major part of the oil discoveries throughout the world in the past twenty years. Today's modern way of life could never have arrived without the abundance of oil discoveries brought about by these "tools." And the United States would have been hard pressed, if indeed it had been able, without these discoveries to supply the oil which won the Second World War for our allies and ourselves. Yes, the history of our twentieth century would have been quite different without them.

DeGolyer was a restless, energetic, inquiring man whose interests were far broader than geology, the art of prospecting for oil, or the entire world oil business. He was an omnivorous reader, especially on subjects which appealed to him. Two of these were the history of science and the early history of the southwestern United States. His interest led him to assemble one of the finest libraries on the history of science in the western hemisphere. This he generously donated to the University of Oklahoma. It now contains more than 15,000 items, including some of the oldest books about the earth, such as *Study of the Universe* by Hbranus Maurus printed in 1467, an edition of Euclid's *Geometry* published in 1482, five first editions of Galileo's works, Newton's *Principia*, and other items of importance down to "modern" works of the best authors. Here in one collection are the landmarks, the books which characterize the major steps of science in the past five hundred years. His library on the history of the Southwest remains at his home and is becoming a much-sought reference and research collection. A lesser group of books on English poetry is housed at the University of Texas.

DeGolyer had a complex, many-sided personality. In conversation and correspondence about him with his friends and associates, the most frequent comments dwell on the character and quality of his mind. Such phrases as "a superb intelligence," "great mental courage," "brilliant qualities of mind," "logical reasoning," "clear thinking," and "sound judgment" are typical. But many say that his greatest talent lay in his understanding and appreciation of other people's minds, an ability to discern a good idea expressed by someone else, to enlarge upon it, even to stimulate the author of the idea to a more fully rounded understanding of his own idea. Without doubt he wielded a great influence and was a fine stimulator of other people's minds and ideas. He was truly a catalyst in many mental processes.

A man with such a fine intellect would of course have self-confidence, and an ample quantity of this characteristic is what no doubt led some to apply to him the words "cocky," "vain"—and other similar epithets. Others described him as having "a vast sense of self-

importance" and even a lack of tolerance for those less gifted. Perhaps he was even selfish or bordering on the ruthless in his more aggressive undertakings. Certain it was that he hated to lose an argument, a dollar, or a game of cards—when engaged in the latter he could seldom be persuaded to quit until he was winning.

Right alongside these descriptions we find such phrases as "a quick penetrating wit," "marvelous sense of humor," "good mixer," "makes friends wherever he goes," "thoughtful," "gracious and friendly," "a storyteller beyond compare," and "a most stimulating man." But friends and foes alike would agree that although he on occasion could be tough and stubborn, through it all he was inquiring and penetrating. Such were the diverse talents and multiple personality of the man. Possibly the sum of this complex personality is revealed by the manner of his death. Troubled for more than eight years by aplastic anemia which required constant blood replacement, and having lost the sight of one eye through retinal failure, he sensed the approach of death and, in characteristic fashion, did not wait, but went out to meet it.

Speaking on the occasion of the award of the Sidney Powers Medal of the American Association of Petroleum Geologists in May, 1950, Carey Croneis said, ". . . few people in any profession now or at any time in the past have been so honored during the productive years as Everette Lee DeGolyer. And this is perhaps the true measure of the man. For genius is easy to recognize—or even fabricate—in retrospect; it is difficult to discover—and, once discovered, sometimes irksome to acknowledge—in contemporary colleagues and especially fellow workers. But not so for DeGolyer—the recognition of his talents came early and soon became well nigh universal."

In addition to the Sidney Powers Memorial Medal, the highest honor given by the Petroleum Geologists, he was awarded two of the highest engineering medals—in 1941 the Lucas Medal of the A.I.M.E., and in 1942 the John Fritz Medal of the four Founder Engineering Societies. He was in 1925 the president of the American Association of Petroleum Geologists, and in 1927 became the first

“oil man” to be president of the American Institute of Mining and Metallurgical Engineers.

He was much in demand as a lecturer and delivered the Aldred lectures at the Massachusetts Institute of Technology in 1929, the Brackett (1929) and Vanuxem (1941) lectures at Princeton University, and served the University of Texas as visiting Distinguished Professor in 1940. He gave commencement addresses at the Colorado School of Mines in 1925, Trinity College in 1947, and the University of Oklahoma in 1948. During October and November of 1947 he made a tour under the sponsorship of the A.A.P.G. Distinguished Lecture Committee and spoke before forty-four geological societies and university groups on the subject “Prospecting for Petroleum.”

He was honored with the degree of Doctor of Science by the Colorado School of Mines, Southern Methodist University and Washington University, and the Doctor of Laws degree by Trinity College, Tulane University, and the Universidad de Mexico. In 1948 he was given the first Distinguished Service Citation by the University of Oklahoma and in 1952 was inducted into the Hall of Fame of Oklahoma. He was elected to the National Academy of Sciences on April 25, 1951, and at the time of his death he was a Regent of the Smithsonian Institution. A further demonstration of his interest in higher education was his service as chairman of the Gulf District Committee of Selections for Rhodes Scholarships for ten years.

His membership in geological, engineering, and other technical societies included nearly all those in the western and eastern hemispheres. He was an honorary member of the A.A.P.G. and A.I.M.E., as well as the Society of Exploration Geophysicists. He was given the Distinguished Service Citation of the Texas Mid-Continent Oil and Gas Association in 1939. He was honored by alumni membership in Phi Beta Kappa, Tau Beta Pi, Sigma Xi, Sigma Gamma Epsilon, and other honorary fraternities. His social fraternity while in college was Kappa Alpha (So.).

DeGolyer was the author of many articles and papers—perhaps several hundred in all. The more important are listed in the bibli-

ography at the end of this memoir. In his chosen field his most significant contributions were studies in early years on the nature of salt domes, and in later years on the art of prospecting for petroleum. He was editor (1940) of a notable monograph, *Elements of the Petroleum Industry*, published by the A.I.M.E. As an indication of the diversity of his writings, he was the author of the biography of the Mexican general Santa Anna in the *Encyclopaedia Britannica*.

As would be expected of a man of such outstanding reputation, his services were called for by the Federal Government. Starting in 1918 with a special report for the United States Treasury, he became in 1941 Director of Conservation in the Office of the Coordinator for National Defense and Assistant Deputy Coordinator in 1942. The following year he was first made Assistant Deputy Administrator for War, and then head of the Petroleum Reserves Corporation mission to the Middle East. He was a member of the Advisory Committee on Raw Materials for the Atomic Energy Commission, and filled many other important positions. He served on the National Petroleum Council from its beginning.

On the business side of his activities he had been during his career an officer or director of more than a score of companies, and was active in ten or more at his death. He had a keen sense of duty and loyalty to his city and state, and served in many capacities, including president of the Dallas Art Association, president of the Dallas Arboretum Foundation, and vice-president of the Dallas Historical Society.

In 1910, when DeGolyer returned from his highly successful hunt for oil in Mexico to the University of Oklahoma campus to finish his work for a degree, he was given a retainer of \$500 per month by his employers, the Mexican Eagle Oil Company, and was rated the "richest" student on that small and quite new university campus. At the close of the school year he married the girl whom he had met on his previous sojourn at the University when she was a student and he was "sharing a Sunday suit" with his roommate. Nell Goodrich DeGolyer, his wife, survives him, as do three daughters, one son, and fifteen grandchildren.

KEY TO ABBREVIATIONS

- A. A. P. G. Bull.=The American Association of Petroleum Geologists Bulletin
 A. I. M. E. Bull.=American Institute of Mining and Metallurgical Engineers Bulletin
 A. I. M. E. Trans.=American Institute of Mining and Metallurgical Engineers Transactions
 Amer. Pet. Inst. Bull.=American Petroleum Institute Bulletin
 Asoc. Mexicana Geol. Petroleros Bol.=Asociacion Mexicana de Geologos Petroleros Boletin
 Auto. Ind.=Automotive Industries
 Econ. Geol.=Economic Geology
 Geol. Soc. Amer. Bull.=Geological Society of America Bulletin
 Jour. Geol.=Journal of Geology
 Min. & Met.=Mining and Metallurgy
 Nat. Pet. News=National Petroleum News
 Oil Eng. Fin.=Oil Engineering and Finance
 O. & G. Jour.=The Oil and Gas Journal
 Pet. Eng.=Petroleum Engineer
 Pet. Rev.=Petroleum Review
 Sat. Rev. Lit.=Saturday Review of Literature
 Soc. Pet. Geophy. Jour.=Society of Petroleum Geophysicists Journal
 U. S. G. S.=United States Geological Survey

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L.O. Howard

LELAND OSSIAN HOWARD

June 11, 1857–May 1, 1950

BY JOHN E. GRAF AND DOROTHY W. GRAF

TO APPRECIATE THE ACCOMPLISHMENTS of Dr. Howard in his chosen profession, it is necessary to understand the period spanned by his lifetime. In his youth the dramatic and all-conquering march of the potato beetle from the eastern foothills of the Rockies to the Atlantic was still fresh in memory. The first salaried entomologist of the United States Government was appointed in 1854. The science of entomology developed slowly, and when Dr. Howard came to Washington in 1878 there were few trained investigators, little was known of the biology of insects, and control measures were most rudimentary. Nor could much useful information be secured from abroad. During his active career, the nation suffered losses from the so-called Rocky Mountain locust and from invasions of such serious pests of foreign origin as the boll weevil, gypsy moth, Japanese beetle, and the European corn borer. It was a period during which it was necessary to dramatize the importance of economic entomology to make the general public appreciate the seriousness of the problem, and at the same time to develop a wide-flung program of research so that damage from insects could be held to a minimum and crop and animal production made profitable, to insure a greatly expanded agriculture for our rapidly growing country. The problem called for a man of many talents, and fortunately Dr. Leland Ossian Howard was selected for the position of responsibility.

FAMILY HISTORY

Dr. Howard was born in Rockford, Illinois, June 11, 1857, while his mother and father were there on a short stay. His father was Ossian Gregory Howard, a lawyer by profession, and his mother was the former Lucy Dunham Thurber, a singer and teacher of both voice and piano. Both parents had grown up in the town of Delhi, in Delaware County, New York. His paternal grandfather was Dr. Calvin Howard, an old-fashioned country physician, who was admitted to practice about 1817, and who in 1846 was given the honorary degree of Doctor in Medicine by the Columbian Medical College, Washington, D. C. (a strange coincidence being that the same honorary degree was given to his grandson by the same institution, which had changed its name to the Medical College of George Washington University, sixty-five years later). He was instrumental in forming in Delhi, in 1833, The Chemical and Geological Society, afterwards known as The Lyceum of Natural History. Dr. Howard's great-grandmother on his mother's side was Lois Pickering of the remarkable Pickering family of Salem, Massachusetts, to which belonged many distinguished men in science and other walks of life, one of the best known being Timothy Pickering, one of the signers of the Declaration of Independence. Lois Pickering was very advanced for the period in which she lived, extremely independent, and quite a character. She and Abner Thurber eloped and she rode pillion behind him all the way from Salem to the wilds of New York state, where they settled in the small colony which afterwards became Cooperstown. There she later taught school and had as one of her pupils young James Fenimore Cooper. The son of the Thurburs, also named Abner, married Lucy Dunham, of *Mayflower* descent, and they moved to Delhi, where they had a farm and brought up a large family of children, the youngest daughter being Dr. Howard's mother. His grandfather Thurber was not only a farmer and a "hatter," but also an ardent amateur astronomer and something of a poet and a mystic. All of his children were taught to sing

and to love beauty. He was a deacon in the Presbyterian church and his farm was a known station on the Underground Railroad for escaping slaves. His wife, Lucy Dunham, was practical, hard-working, and rather puritanical.

Among Dr. Howard's ancestors on both sides are found lawyers, physicians, clergymen, and farmers with strong leanings to the professions. Other relatives were General Oliver O. Howard, a well-known officer of the Civil War; U. S. Senator Jacob M. Howard of Michigan, who is credited with writing the first platform of the Republican party; Senator Howard's son, James Leland Howard, a former lieutenant governor of Connecticut; Alphonso Taft, a member of President Grant's cabinet, and Henry L. Stimson, Secretary of War under President Taft.

YOUTH

When he was less than a year old, Dr. Howard's parents moved to Ithaca, New York, then a village of seven or eight thousand inhabitants. There his father began to practice law.

The life of the village was very simple in those days, emphasis being put on church affairs, and little outside entertainment was available except for an occasional traveling circus. The Howard family were great readers, who subscribed to the *Atlantic Monthly*, *Harper's Magazine*, and the *New York Tribune*, and read all the better novels as they appeared. Dr. Howard learned to read at a very early age, and also to play whist, so that he could make a fourth when needed.

When he was about six or seven he went on a visit to Long Island where he met some boys who were collecting cocoons. He became interested at once and began his collection of insects, which he continued for fourteen years. His enthusiasm stirred the interest of some of his boy friends and they formed The Ithaca Natural History Society, of which Howard was the first president. There is a photograph of the group, showing him as the only one with a hat, which he said was worn to show that he held this high office.

He swam and boated on Cayuga Lake, camped frequently in the woods, and led the normal boy's life in spite of the very puritanical attitude of his grandmother, who made him promise at an early age that he would never learn to dance. She promised that if he agreed, and also would never join a secret society, she would give him a hundred dollars. Apparently the promises were not too difficult to keep and he got his hundred dollars.

He was very fond of singing, and after his father's death, he used to escort his mother to church, where she was the leading soprano, and if any special part of the choir were missing, he would oblige by singing tenor, baritone, or bass, as needed. When he grew up and came to Washington, he joined the Choral Society, and it was there he first saw the girl who later became his wife.

EDUCATION

Howard first attended a private primary school, then he went to the public schools, and later to the Ithaca Academy. He studied for a year in a private school in Ithaca, and then entered Cornell University, which had been established in Ithaca when he was ten years old. While at Cornell, he came in contact with many nonresident lecturers, including such prominent men as Bayard Taylor, James Russell Lowell, and Louis Agassiz. In the University he was attracted strongly to the whole field of natural history. He continued collecting insects throughout his college career and undertook special studies in the Department of Invertebrate Zoology, spending much time in the laboratory of Professor J. H. Comstock. He also maintained his intense interest in outdoor sports, playing football and baseball, and rowing in one or two crews, though he never gained high rank in college athletics. He graduated from Cornell in 1877 with the degree of Bachelor of Science, afterwards taking one post-graduate course. Four years later he was awarded the degree of M. S. on submittal of a thesis, and taking a special examination in advanced botany.

PROFESSIONAL CAREER

In 1878 Dr. Howard received the appointment of assistant entomologist in the Department of Agriculture. He remained the first assistant entomologist until June, 1894, when he was made chief entomologist, a position which he held until 1927. At that time he requested that he be relieved of administrative responsibility. He then became principal entomologist, in which position he was able to devote full time to those scientific activities in which he was pre-eminent. He retired from government service in 1931 after fifty-three years of service. In 1904 he was made consulting entomologist of the Public Health and Marine Hospital Service, now the U. S. Public Health Service. In 1919 he was senior entomologist with the grade of senior surgeon, U. S. Public Health Service.

At the beginning his office was located in the old brick Department of Agriculture building in Washington at the foot of 13th Street. In those days the staff of the division was very small, and a considerable portion of the work was devoted to answering inquiries. Dr. Howard, however, was greatly interested in insect parasites, and did a great deal of taxonomic work on the parasitic Hymenoptera. As assistant to Dr. Riley, the chief entomologist, his duties were many and varied. Important among them were preparing or assisting in preparing entomological material for publication in the department bulletins and assisting in the editing of *Insect Life*. When Professor Riley resigned to head the U. S. Entomological Commission for a two-year period, Dr. Howard again was able to renew his association with Professor Comstock and his wife, and with William Trelease, the entomologist and botanist, a friendship which made for a happy social as well as professional life. Of this period he writes, "We were filled with the enthusiasm of youth, worked hard, and were delighted with the interesting and important discoveries we made." Dr. Howard gradually took a more important part in the writing of articles, preparing many of these himself, and finally became the editor of *Insect Life*. He was appointed chief of the Divi-

sion of Entomology in 1894 upon the resignation of Dr. Riley. By that time this division, while still small, had grown in prestige, and entomological problems were becoming more and more important as the destructiveness of native pests was recognized and foreign pests invaded the country. The boll weevil had crossed our southern border into the cotton belt of Texas, and the gypsy moth and San José scale which had gained entrance into this country somewhat earlier were recognized as serious insect pests.

The total appropriation for the Division of Entomology when Dr. Howard assumed its leadership was \$30,000, but after the public became conscious of the importance of the study of economic entomology, appropriations increased rapidly and the organization grew with the increase in funds. The Division of Entomology was made a bureau in 1904, adding greatly to Dr. Howard's administrative duties. He still found time, however, to carry on his research work, with emphasis on the parasites of scale insects. All told he described 47 new genera and 272 new species of insects, not including 22 species of mosquitoes credited to him and other workers.

His taxonomic work on parasites greatly increased his interest in their economic utilization, and he was one of the early leaders in the field of biological control of insect pests. This led to the importation of parasites and predators from foreign countries and their colonization here. His work on the parasites of the gypsy and brown-tailed moths is still one of the classics dealing with this subject.

Dr. Howard had taken a premedical course at Cornell, and this led naturally to night school medical studies at the old Columbian University, now George Washington University. His great interest and very considerable knowledge in the fields of entomology and medicine led to a combination of these two disciplines in the field of medical entomology. It was not long before he was a recognized authority on the subject, and important contributions to this field resulted in no small part from his ability to dramatize the subject. The slogan "Swat the Fly" caught the public fancy and resulted in anti-fly campaigns around the world. His publication, *The House*

Fly—Disease Carrier, removed the house fly permanently from the role it hitherto held in childhood stories. He not only called attention to the danger of house flies as disease carriers, but pointed out that the best control was the elimination of breeding places, a remedy which still holds high rank in spite of numerous insecticides which have been developed since his day. He also played an important role in the crusade against mosquitoes.

In like manner, Dr. Howard has dramatized the war between man and insects, and has brought home to people both in this country and abroad the importance of this never ending conflict.

Throughout his professional career Dr. Howard traveled widely. While many of these trips were made specifically for the purpose of arranging for the collection and introduction of foreign parasites, Dr. Howard had the valuable knack of making and holding friends, by means of which he established happy, cooperative relations with the leading entomologists of Europe, a most important factor in the international development of the science of economic entomology.

He recognized the importance of keeping abreast of the literature in his field and was a student throughout his life. He was in the library of the Department of Agriculture almost daily and scanned the current publications, including those which were not the object of his special study. He gave strong support to the librarians and must be given much credit for the excellent library service provided for the professional staff.

Having served four years beyond the normal retirement age, he left the Government service on June 30, 1931, at the age of seventy-four years. He had seen the Division of Entomology grow from an office with an annual appropriation of \$30,000 in 1894, when he became the chief entomologist, to a large and important bureau when he retired as chief in 1927, with appropriations of \$3,000,000, and a wide-flung group of research laboratories in this country and abroad.

In addition to his professional position as chief of the Bureau of Entomology, he was honorary curator of the Division of Insects in the United States National Museum for life, an honor he greatly

appreciated; chairman, Subcommittee on Medical Entomology, National Research Council; consulting entomologist, U. S. Public Health Service; and member of the Committee on Agriculture, National Council of Defense.

In Washington, Dr. Howard was able to find time to pursue his love for outdoor life and sports. In his early days he was a member of the Capitol Bicycle Club and took many long trips on the old "ordinary" bicycles. He was well-known as a racer.

RELATED ACTIVITIES

Dr. Howard took significant and active part in the activities of two groups which played an important role in the development of science in America. The first of these was the American Association for the Advancement of Science, an organization of which he was secretary of Section F in 1893; vice-president and chairman of the same section in 1895 and 1897; and in 1898 was elected permanent secretary, a position he held for twenty-two years. In 1920 he was elected president. The second of these groups was the Cosmos Club, of which he was secretary from 1894 to 1907, president in 1909, and, in addition, held other offices. The Cosmos Club was really his second home. The positions he held there naturally required his frequent presence at the Club but he went there daily because he liked those who made up its membership and was at ease when he was with them whether having a lively discussion of some subject of importance or enjoying a pleasant game of billiards or cowboy pool. Dr. Howard was also an excellent bridge player. In all the games he played he possessed not only a high degree of skill but strong competitive spirit. As a result of his long service as secretary of the Cosmos Club and permanent secretary of the American Association for the Advancement of Science, he undoubtedly knew more American and European scientists than any one of his generation. He was at his best as an ambassador of science, whether at home or abroad.

He was also greatly interested in international scientific meetings

and in the international congresses of entomology and zoology he always took a prominent part.

HIS FAMILY

In 1886 Dr. Howard was married to Miss Marie Theodora Clifton. Her family came originally from Baltimore, but moved to Washington when she was a child. Her father's family was very musical, her grandfather having been a teacher, violinist, and composer, and one of her aunts a well-known singer in both Baltimore and Washington. One of her grandfather's compositions was the march played at the opening of the Baltimore and Ohio Railroad, which was written especially for that occasion. Mrs. Howard had a very beautiful voice herself, and for some years before her marriage was leading soprano at St. Matthew's Church.

The early years of their married life were spent in a house in Georgetown where their three daughters, Lucy Thurber Howard, Candace Leland Howard, and Janet Moore Howard, were all born. There they spent their winters, going to the Catskill Mountains in the summer, to a pleasant summer colony founded by Dr. Howard's uncle, Mr. Francis B. Thurber of New York, and his aunt, Mrs. Candace Wheeler. During his active life, Dr. Howard was able to spend but little time there, but when there on vacation, he was able to study in a leisurely fashion the habits of some of his insect friends. Later, when he had more time, he spent pleasant hours on the golf course.

His family life was very happy. He was no disciplinarian, leaving that chore to his wife, except for an occasional word of warning. But as his daughters grew up they found in him a most delightful companion, who revealed to them the joys of the books he had read and loved.

Mrs. Howard died in 1926, and in 1933 his daughter Candace, who had been married in 1925 to Edward de Mille Payne of New York, died at the birth of her only child, a boy who was named after his grandfather, Leland Howard Payne.

PRINCIPAL ACCOMPLISHMENTS

Opinions may vary as to the relative importance of the fields which held Dr. Howard's interest, but it seems clear that, from the standpoint of permanent contributions to science, he will be remembered (1) for his taxonomic work on parasitic insects, (2) for his part in the encouragement of biological control of insects, (3) for the early stimulus he gave to the subject of medical entomology, (4) for his foreign contacts in economic entomology, and (5) for his leadership in portraying the insect problem.

Since the introduction of parasites for the control of insect pests, this work has expanded and attained increasing importance, in spite of recent developments of many new insecticides. Several spectacular successes and others in which partial control is effected give ample proof that biological control will continue to gain support as an important contribution to insect control.

In the early days of medical entomology, Dr. Howard demonstrated that entomologists had an important place in the investigation and control of insect-borne diseases. This has contributed in an important measure to the steady growth of medical entomology, both in the promotion of general public health and in the protection of military personnel.

Dr. Howard's part in making economic entomology truly international through his foreign contacts and his participation in international meetings and congresses may be shown by quoting from a statement made by the British scientist, R. Stewart MacDougall, on the occasion of the Third International Congress of Entomology in Zurich. "A special tribute must be paid to Dr. L. O. Howard, of the United States Department of Entomology. Always in a position to help and using his position to help, Dr. Howard has earned the gratitude of entomologists everywhere. Capable and kindly, always with the right word, and with tact as his middle name, Howard gives one the feeling that were there a dozen representative ambassadors like him in the political world, we would soon have what some

of us long for, the United States of Europe, each nation no longer at enmity with the other but working out its own salvation following the lines of its own culture and psychology. Certainly there was a spirit of friendliness and goodwill at the Congress, attesting that science has no limited boundaries but is international."

Dr. Howard led the move to point out the losses insects caused to man's health, his food, and his home. In so doing he obtained sufficient support not only to advance the science of entomology but to greatly accelerate the research on injurious insects. In this effort he went directly to the people and used methods which would drive him to his objective, whether it was speeches or popular articles in farm periodicals, magazines of wide circulation, or newspapers. That this crusade was successful is shown by the steady and continuing growth of the Bureau of Entomology. This is probably his greatest contribution to science, for it served to multiple his own efforts many fold.

PERSONAL QUALITIES

Of his father, Dr. Howard has noted, "He was brilliant, was a great raconteur, and had a biting wit." These qualities Dr. Howard inherited, since he had a brilliant mind, was an excellent raconteur and speaker, had a keen sense of humor, and, on those occasions when he felt the circumstances justified biting wit, could use it. As a speaker, he cared little whether his audience was composed of children or adults, scientists or laymen. He liked all people and he charmed them. As an administrator, he was not a driver but preferred to select the right man for the job and then provide encouragement and support. He disliked contention and always sought to resolve differences without dispute. At an early period he had surrounded himself with a group of principal advisers, including C. L. Marlatt, his first assistant, A. L. Quaintance, and W. D. Hunter. With this "cabinet" supporting the chief the Bureau of Entomology did not lack for sound administrative guidance on a high level.

Modesty always characterized his endeavors. In writing of his ac-

complishments in the fiftieth anniversary book compiled by the class of 1877 of Cornell University, he says: "Circumstances have conspired to emphasize the importance of economic entomology, and a great service has been built up through the munificence of Congress and the cordial help and appreciation of very many helpfully appreciative people all over the country. I have been especially interested, aside from the general field of economic entomology, in parasitism among insects, and in the comparatively new field of medical entomology. I have written several books and about eight hundred lesser papers, and, largely owing to the sound and important work done by my assistants and associates, have, as the chief of the service, received many honorary memberships in foreign academies and societies, several honorary degrees, and that sort of thing" (*Cornellian Council Bulletin*, March, 1931).

"I have made no especial discoveries" (from Dr. Howard's papers deposited with the National Academy of Sciences).

On the occasion of a visit to the Whittier, California, citrus insect laboratory, after listening to Mr. R. S. Woglum describe his research in the control of citrus insects, he turned to his traveling companion, Dr. Marchal, the distinguished French entomologist, and said, "You can see from the fine work of these men that it is an easy task to be chief of the Bureau of Entomology."

When he was asked to what outstanding accomplishment the Capper Medal and Award might be attributed he said he did not know, but that the real credit belonged to his aides.

These modest sentiments show why the men who served under Dr. Howard will always remember with gratitude his generosity in recognizing their contributions.

His friends will always remember him for his tireless industry in driving towards his goal, his affectionate regard for his family and friends, his keen sense of humor, and his interest in people generally, whether at work or play.

HONORS

Dr. Howard received several honorary degrees, including Honorary Ph.D., Georgetown University, 1896; Honorary M.D., George Washington University, 1911; L.L.D., University of Pittsburgh, 1911; L.L.D., University of California, 1929; Sc.D., University of Toronto, 1920; Sc.D., Rutgers University, 1930. He was a trustee of Cornell University, 1900-1905.

Probably one of his greatest thrills came to Dr. Howard in 1931 when he was the recipient of the Capper Award, consisting of a gold medal and a cash award of \$5,000. Other high honors were: Chevalier de la Légion d'Honneur, France; Officier de la Légion d'Honneur, France; Officier de l'Ordre du Merite Agricole, France; Gold Medal, Italian Ministry of Agriculture (Al Merito Agricola); Gold medal, Holland Society of New York; Buffon Medal, Museum d'Histoire Naturelle, Paris; 1st medalist, New York Farmer.

As might be expected of one who was deeply interested in spreading the knowledge of economic entomology around the world, Dr. Howard attended a number of international congresses as well as other foreign conferences in which the United States had special interest. He was a delegate, International Congress of Agriculture, Vienna, 1907; chairman, Washington section International Congress of Zoology, Washington, 1907; delegate, Lamarck Centenary Fete, Paris, 1903; delegate, Darwin Centenary Celebration, Cambridge, England, 1908; chairman, section of Economic Entomology, 2nd International Congress of Entomology, Oxford, England, 1912; Honorary President, International Congress of Phytopathologists and Economic Entomologists, Wageningen, Holland, 1923; vice-president, International Congress of Agriculture, Paris, 1923; delegate, Olive Fly Conference, Madrid, 1923; chairman, Pan-Pacific Conservation Conference, Honolulu, 1924; chairman, general session, 3rd International Entomological Congress, Zurich, 1925; delegate, International Congress of Agriculture, Warsaw, 1925; president, section of Economic Zoology, International Congress of Zool-

ogy, Budapest, 1927; president, Fourth International Congress of Entomology, Ithaca, N. Y., August, 1928; honorary member, V Congres International d'Entomologie, Paris, 1932.

Dr. Howard was proud of his close friendships with the entomologists of many foreign countries. His standing in this regard was shown by his election to honorary membership in foreign societies, most of which were scientific and all of which had high qualifications. He was an honorary member of the Entomologiska Foreningen in Stockholm; Trinidad Field Naturalists Club; Sociedad cientifica "Antonio Alzate," Mexico; Ontario Entomological Society; Real Liga Agraria do Norte, Portugal; Allgemeines Entomologische Gesellschaft, Germany; Entomological Society of Ontario; Entomological Society of the Northwest Provinces; Société Entomologique de France; Association of Economic Biologists of Great Britain; Société Nationale d'Acclimation de France; Society for the Destruction of Vermin, London; Deutsche Gesellschaft für Angewandte Entomologie; South African Biological Society; Société Scientifique du Chile; Société Entomologique de la Russie; Sociedad de Estudios Biologicos, Mexico; Entomological Society of Moscow; Scientific Council Board of the Russian Bureau of Entomology; Société d'Etude et de Vulgarisation de la Zoologie Agricole, France; Academie Chilena de Ciencias Naturales; Sociedad Chilena de Historia Natural; Czechoslovak Academy of Agriculture; Société Linnéenne de Bordeaux; corresponding member, Institut National Genevois, Switzerland; correspondent, Museum d'Histoire Naturelle, Paris; member, Commission International d'Agriculture; Académie d'Agriculture de France; fellow, Entomological Society of London, England; foreign correspondent, Real Academia de Ciencias y Artes de Barcelona; colaborador de la Dirección de Estudios Biologicos, Mexico; corresponding member, Zoological Society of London; Entomological Society of Chile.

Dr. Howard was a member or honorary member of many American scientific societies. These include National Academy of Sciences; American Philosophical Society; American Academy of Arts and

Sciences (Fellow); American Association for the Advancement of Science (Permanent Secretary 1898-1920, President, 1920); Entomological Society of America (Fellow); Member American Institute of Social Sciences; Washington Academy of Science (President, 1916); Academy of Natural Sciences of Philadelphia (Correspondent); American Entomological Society (Corresponding member); Staten Island Association of Arts and Sciences (Corresponding member); American Association of Economic Entomologists (President, 1894); Entomological Society of Washington (President and Honorary President); Honorary member California Academy of Sciences; Davenport Academy of Natural Sciences; American Society of Tropical Medicine; Entomological Society of Albany; Harris Club of Boston; New Jersey Mosquito Extermination Association; Louisiana Entomological Society; Maryland Academy of Sciences; New York Entomological Society; Brooklyn Entomological Society; Entomological Society of America (Honorary Fellow); Honorary chairman, National Malaria Committee (1925).

This impressive list of honors received and membership in scientific groups is ample evidence of the high esteem in which Dr. Howard was held in cultural and scientific circles abroad as well as in this country.

LAST YEARS

The last years of Dr. Howard's life were spent very quietly. He had moved away from Washington at the beginning of the Second World War and spent the last eight years of his life in one of the suburbs of New York City. He had been physically incapacitated by a fall, but he was mentally keen and active to within a very short time of his death. He read the scientific papers and journals with great interest, and kept in as close touch as he could with the work of the Bureau of Entomology and international entomology. His correspondence with friends and colleagues all over the world was a joy to him. He must have missed Washington and his work greatly,

but he never spoke of it or complained. He died peacefully within a month of his ninety-third birthday, on May 1, 1950.

No more accurate summary could be made to the great contributions of this eminent scientist and scholar and dynamic crusader than to quote from the obituary by Dr. F. C. Bishopp: "On May 1, 1950, the long and brilliant career of Leland Ossian Howard, the world's most outstanding entomologist, came to an end. But the impact of his contributions as a research worker, writer, educator, and administrator will continue to be felt all over the globe for many years to come."

Dr. Howard's daughter, Miss Lucy T. Howard, kindly read the manuscript and rewrote and added materially to the sections "Family History," "Youth," and "His Family," and wrote "Last Years." The clippings, papers, and bibliography maintained over many years by the late Misses Mabel Colcord and Ina Hawes, formerly librarians of the Bureau of Entomology, were of great assistance. Mrs. Ann Campbell of the Smithsonian Institution contributed materially to the arrangement and editing of the biography.

KEY TO ABBREVIATIONS

- Amer. Assoc. Adv. Sci. Proc.=American Association for the Advancement of Science Proceedings
 Amer. Ent. Soc. Trans.=American Entomological Society Transactions
 Amer. Jour. Pub. Health=American Journal of Public Health
 Amer. Nat.=American Naturalist
 Amer. Pomol. Soc. Proc.=American Pomological Society Proceedings
 Argentina Min. Agr. Bol.=Argentina Ministerio de Agricultura Boletín
 Can. Ent.=Canadian Entomologist
 Carnegie Inst. Wash. Pub.=Carnegie Institution of Washington Publication
 Carolina Med. Jour.=Carolina Medical Journal
 Centbl. f. Bakt., Parasitenk. u. Infektionskrank., Abdruck=Centralblatt für Bakteriologie, Parasitenkunde und Infektionskrankheiten, Abdruck
 Chem. Age=Chemical Age
 Country Gent.=Country Gentleman
 Eng. News-Rec.=Engineering News-Record
 Ent. Amer.=Entomologica Americana
 Ent. News=Entomological News
 Ent. Soc. Amer. Ann.=Entomological Society of America Annals
 Ent. Soc. Ontario Ann. Rept.=Entomological Society of Ontario Annual Report
 Hawaiian Ent. Soc. Proc.=Hawaiian Entomological Society Proceedings
 Inter. Cong. Ent. Proc.=International Congress of Entomology Proceedings
 Internatl. Conf. Phytopath. Econ. Ent. Rept.=International Conference of Phytopathology and Economic Entomology Report
 Internatl. Zool. Cong. Proc.=International Zoological Congress Proceedings
 Jour. Econ. Ent.=Journal of Economic Entomology
 Linn. Soc. London Jour. Zool.=Linnean Society of London Journal of Zoology
 Mass. State Forest. Ann. Rept.=Massachusetts State Forester, Annual Report
 Md. Acad. Sci. Jour.=Maryland Academy of Sciences Journal
 Mich. State Hort. Soc. Ann. Rept.=Michigan State Horticultural Society Annual Report.
 Natl. Acad. Sci. Proc.=National Academy of Science Proceedings
 Natl. Geog. Mag.=National Geographic Magazine
 N. J. Mosquito Exter. Assoc. Proc.=New Jersey Mosquito Extermination Association Proceedings
 N. Y. Med. Jour.=New York Medical Journal
 Pop. Sci. Monthly=Popular Science Monthly
 Rev. Chilena Hist. Nat.=Revista Chilena de Historia Natural
 Rev. Rev.=Review of Reviews
 Royal Ent. Soc. London Proc.=Royal Entomological Society of London Proceedings
 Sci. Amer.=Scientific American

- Sci. Monthly=Scientific Monthly
 Smithsonian Misc. Coll.=Smithsonian Miscellaneous Collections
 Soc. Ent. France Bull.=Société Entomologique de France Bulletin
 Soc. Natl. d'Acclim. de France Bull.=Société National d'Acclimation de France Bulletin
 U. S. Com. Agr. Rept.=U. S. Commissioner of Agriculture Report
 U. S. Dept. Agr. Div. Ent. Bull.=U. S. Department of Agriculture Division of Entomology Bulletin
 U. S. Dept. Agr. Div. Ent. Cir.=U. S. Department of Agriculture Division of Entomology Circular
 U. S. Dept. Agr. Div. Ent. Tech. Bull.=U. S. Department of Agriculture Division of Entomology Technical Bulletin
 U. S. Dept. Agr. Div. Ent. Tech. Ser.=U. S. Department of Agriculture Division of Entomology Technical Series
 U. S. Dept. Agr. Farmers' Bull.=U. S. Department of Agriculture Farmers' Bulletin
 U. S. Dept. Agr. Rept.=U. S. Department of Agriculture Report
 U. S. Dept. Agr. Sec. Off. Cir.=U. S. Department of Agriculture Secretary's Office Circular
 U. S. Dept. Agr. Yearbook=U. S. Department of Agriculture Yearbook
 U. S. Natl. Mus. Proc.=U. S. National Museum Proceedings
 U. S. Pub. Health Serv. Repts.=United States Public Health Service Reports
 Wash. Acad. Sci. Jour.=Washington Academy of Sciences Journal
 Wash. Acad. Sci. Proc.=Washington Academy of Sciences Proceedings
 Wash. Biol. Soc. Proc.=Washington Biological Society Proceedings
 Wash. Ent. Soc. Proc.=Washington Entomological Society Proceedings
 Wyo. State Bd. Health Bien. Rept.=Wyoming State Board of Health Biennial Report

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1881

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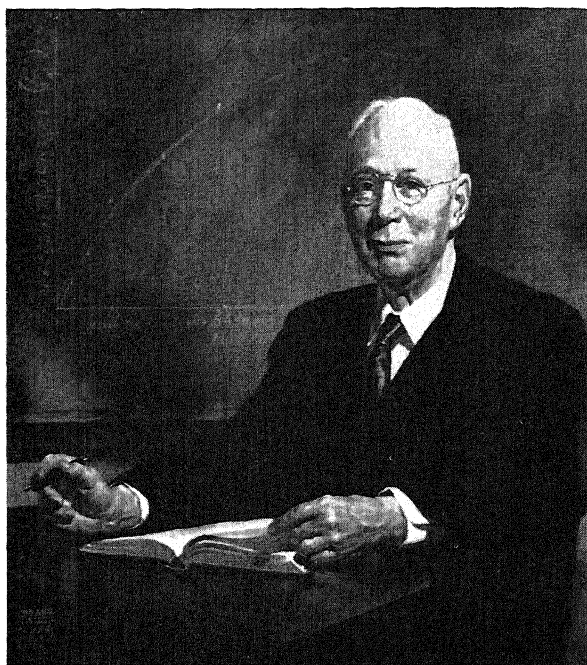
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Clark L. Hull

CLARK LEONARD HULL

May 24, 1884–May 10, 1952

BY FRANK A. BEACH

CLARK LEONARD HULL was born in a log farmhouse near Akron, New York, on May 24, 1884. He died in New Haven, Connecticut, on May 10, 1952. He and his younger brother grew up under pioneer conditions, helping their father clear the land of stumps, splitting the rails for fences, and engaging in the usual round of farm chores.

His first paternal ancestor to come to this country was a Church of England clergyman who landed in Massachusetts in 1635. At first the family lived in that state and followed the sea. Later generations were farmers, living at first in Rhode Island and Connecticut, and later in New York State. The family religion was originally Quaker, and Hull has recorded in an autobiographical sketch the fact that one of his great aunts was hanged on Boston Common because of her religious convictions. His parents were apparently not deeply religious and his father had no formal schooling.

Hull himself seems to have had some leaning toward religion at various times in his life, but he was never able to accept it wholeheartedly. His own account of an early attempt to come to grips with religious problems appears in his autobiography.¹

"When I was eleven or twelve years of age, some traveling evangelists known as Christian Crusaders staged a violent revival at the

¹ This and all subsequent quotations referring to Hull are taken from C. L. Hull, *A History of Psychology in Autobiography*, edited by E. G. Boring, H. S. Langfeld, and R. M. Yerkes (1952), IV, 143–62.

local Methodist Church, the only denomination active there at the time, and I, among a dozen or so other boys and girls, was 'converted' and joined the Church on probation. This experience aroused in me for the first time serious thought about religious matters, with the result that before the period of probation ended I had become very doubtful regarding the whole religious hypothesis. I finally announced my conclusions in open meeting and withdrew my affiliation."

Hull's family moved to Michigan and his formal education began in a one-room rural school. He later attended high school for one year in West Saginaw, Michigan, subsequently transferring to the academy of Alma College. At the end of his second year at Alma Academy, Hull suffered an attack of typhoid fever which was nearly fatal. His temperature remained dangerously high for four weeks, leaving him with permanent amnesia for that period and a generalized bad memory for names, according to his own account. Following his convalescence Hull enrolled in Alma College as a freshman and began to prepare for a career as a mining engineer. This plan was abandoned, however, because he contracted poliomyelitis and was left with one leg so badly paralyzed that he could not walk without crutches.

While convalescing from the polio attack, and for the next two years, Hull devoted a great deal of thought to the choice of a life occupation. His account of his own deliberations follows.

"One possibility was that I enter the ministry of the Unitarian Church, which at that time I considered essentially to be a form of free, godless religion. The preparation of ethical sermons of mainly philosophical content seemed attractive, but the contemplation of the probable necessity of attending an endless succession of ladies' teas and related functions led me to abandon this idea. What I really wanted was an occupation allied to philosophy in the sense of involving theory: one which was new enough to permit rapid growth so that a young man would not need to wait for his predecessors to die before his work could find recognition, and one which would

provide an opportunity to design and work with automatic apparatus. Psychology seemed to satisfy this unique set of requirements. Accordingly I made a preliminary survey of the subject by studying about fourteen hundred pages of William James' *Principles of Psychology*."

As soon as his health permitted, Hull and his bride, Bertha Iutzi, entered the University of Michigan, where he majored in psychology. He considered the course in experimental psychology given by Professors W. B. Pillsbury and J. F. Shepard as the outstanding one in his entire career as a student.

Hull won the B.A. degree in 1913, left school to teach for a year, and then began his graduate training in psychology at the University of Wisconsin. Here he was strongly influenced by his contacts with Professors Daniel Starch and V. A. C. Henmon. He was awarded the Ph.D. degree in 1918.

Hull had begun his teaching career before entering college. At seventeen he passed a teacher's examination and was appointed to a one-room country school for one year. After recovering from the attack of poliomyelitis he taught grades 7, 8, 9, and 10 in the rural school which he had attended as a child. Years later Hull wrote of this experience, "This work was deeply satisfying, and I believe that the teaching was the best I've ever done." Following graduation from Michigan, Hull taught for one year in a small normal school in Richmond, Kentucky. This was a less rewarding experience, and Hull was "impressed with the casual attitude of students from the blue grass country, and the generally sterile nature of the faculty."

The next year as a beginning graduate student at the University of Wisconsin he held a part-time teaching assistantship. He was responsible for quiz sessions connected with the elementary course and was also in charge of the laboratory portion of the course in experimental psychology. The amount of teaching Hull did during his career as a graduate student exceeded the assignment of beginning instructors in some universities today. During his second year at Wisconsin he was officially a full-time instructor in the department. He

was asked to take over a course in psychological tests and measurements. He renamed the course "Aptitude Testing" and completely reorganized the syllabus, eventually writing a book on the subject.

During the same year he taught a special course in "Introductory Psychology" for premedical students. In connection with this responsibility Hull became interested in problems of suggestibility, and this eventually led him into a long program of research on hypnosis. Finally, still during his years as a graduate student, Hull lectured on experimental psychology. This was the one course above all others that he wanted to give because he believed it constituted the foundation of a truly scientific psychology.

After earning his advanced degree, Hull remained at Wisconsin as a member of the Department of Psychology. He became intensely interested in the current conflict between American behaviorism and the German Gestalt school. He tried unsuccessfully to get a foreign fellowship to study with Professor Kurt Koffka in Germany. He did, however, manage to arrange for Koffka to spend a year at Wisconsin.

Koffka spent most of his time attacking Watsonian behaviorism; but Hull, instead of being converted to the Gestalt point of view, reached the conclusion that Watson had not made out as good a case for behaviorism as the facts warranted. Hull decided to do this himself. Just before leaving Wisconsin he read Anrap's new translation of Pavlov's book, *Conditioned Reflexes*, and his later thinking was strongly influenced by some of the notions of Pavlov.

In 1929 Hull moved to Yale University as a research professor in the Institute of Psychology, which later became a part of the Institute of Human Relations. Still later Hull was appointed Sterling Professor of Psychology.

In the summer of 1929 Hull taught a course on aptitude testing in the School of Education at Harvard University. Discussions of various scientific concepts with C. I. Lewis and with A. N. Whitehead strengthened Hull's interest in theory-building. At this time he purchased and became thoroughly familiar with Newton's *Principia*,

a work which strongly influenced his thinking from that time on. He also found stimulating the *Principia Mathematica* of Whitehead and Russell.

During his years at Yale he had no formal teaching responsibilities but regularly conducted a small, weekly seminar for graduate students, "in order to keep in contact with the young." The seminars were almost always centered upon Hull's current research and theoretical formulations. Accordingly the content varied from year to year and it was not uncommon for a student to take the seminar several years in succession.

It was about 1930 when Hull finally reached the decision which was to shape his research and teaching for the remainder of his life. He described his own thinking in the following excerpt from his autobiography.

"I came to the definite conclusion . . . that psychology is a true natural science; that its primary laws are expressible quantitatively by means of a moderate number of ordinary equations; that all of the complex behavior of single individuals will ultimately be derivable as secondary laws from (1) these primary laws together with (2) the conditions under which the behavior occurs; and that all the behavior of groups as a whole, i.e., strictly social behavior as such, may similarly be derived as quantitative laws from the same primary equations. With these and similar views as a background, the task of psychologists obviously is that of laying bare these laws as quickly and accurately as possible, particularly the primary laws."

In 1936, together with a few students and associates, Hull instituted a series of evening seminars which came to be known as the "Monday Night Meetings." With the cooperation of Neal Miller, John Dollard, Hobart Mowrer, and others, Hull discussed the essential identities in conditioned reflexes and behavior laws on the one hand and the phenomena dealt with by Freud and his psychoanalytic associates on the other. These meetings aroused much interest on the part of psychologists, sociologists, psychiatrists, and anthropologists, and at times as many as seventy people attended.

Hull's interest in research manifested itself early. As an undergraduate at Michigan he carried out an experiment on learning under the supervision of Professor J. F. Shepard. At that time and throughout his professional career Hull was very much interested in the evolution of concepts. Prior to his entry into graduate school he designed and constructed an automatic memory machine which he later used in the prosecution of his thesis research. Hull's thesis dealt with concept formation.

Another major research project dealt with the effects of tobacco smoking on mental and motor efficiency. Hull devised an ingenious methodology. Control subjects sat in the dark and "smoked" a special pipe which smelled of tobacco but contained instead of tobacco a special metal capsule with an electrical heating element combined with an asbestos moisture-holding device. This pipe provided a close approximation to the sensations of smoking and the artificiality of the situation went unnoticed by all but one of his subjects.

When he took over the course on psychological tests and measurements at Wisconsin, Hull was concerned with the chaotic nature of the available material and with the necessity for test validation. He addressed himself to these problems with characteristic vigor and incisiveness. In addition to inventing new tests of his own, he created test batteries intended to measure various special aptitudes. This necessitated the computation of large numbers of product-moment correlations. To reduce the time and labor involved in these computations Hull designed and constructed a correlation machine which performed nearly all of the arithmetical work automatically.

As a result of his original researches in the field and his thorough examination of the relevant literature, Hull published in 1928 a book entitled *Aptitude Testing*. This definitive volume had an immediate and beneficial effect upon the testing field and is still cited in modern writings.

As has been mentioned, Hull became interested in suggestibility as a consequence of his lectures to premedical students. This interest led to a major research program on hypnosis and other forms of

suggestion. The work covered a span of ten years and yielded more than thirty separate experimental reports. The program was finally summarized in 1933 in the book *Hypnosis and Suggestibility: An Experimental Approach*. This volume is regarded as a classic and is widely studied today.

Following the periods of work on aptitude testing and suggestibility, Hull concentrated for the remainder of his professional life upon learning and behavior theory. For twenty years he labored to construct a theoretical system which has been one of the most stimulating influences upon the American approach to learning during the present century. From 1929 to 1943 Hull worked incessantly upon the ideas which were to appear in his most important scientific contribution. This volume, *Principles of Behavior*, was published in 1943 when its author was fifty-nine years old. It has been one of the most influential books on the theory of learning since the days of Pavlov and Thorndike, if not the most influential.

It was typical of Hull that he continued to develop his system after the book's publication. Students and former associates frequently were deluged with many-paged mimeographed memoranda embodying additions or emendations of the original formulations.

Many of the changes were radical, constituting major revisions, and in 1951 Hull published the *Essentials of Behavior*, in which he put forth an extensively modified theory.

Hull was what might be called a "quantitative thinker," and from his earliest school days he found arithmetic an easy study, while grammar and languages were comparatively difficult. In connection with a description of his high school education he once wrote as follows:

"Indeed, the study of geometry proved to be the most important event of my intellectual life; it opened to me an entirely new world—the fact that thought itself could generate and really prove new relationships from previously possessed elements."

Hull's attitude toward science was characterized by a missionary zeal. He championed the hypothetico-deductive technique and al-

ways strove for rigorous formalization and quantification. His concept of the fruitful approach to behavioral science involved making postulates about untestable functions or processes, and then deriving theorems to test the postulates.

He was an exceedingly critical thinker and could be intolerant of lack of rigor in the thinking of students or colleagues. Even as a graduate student he found reason to complain of lack of clarity and content in the lectures delivered by some of his professors.

"I found Professor 'J' a kindly person to serve under, but I learned little from the years of association with him. His mind could scintillate in a brilliant fashion but his approach to psychology was largely qualitative and literary. [He] had remarkable linguistic fluence. He could sometimes lecture for five minutes at a time in perfectly good sentences, yet hardly saying a thing."

Occasional lapses from rigorous thinking on the part of graduate students were apt to provoke profanity (which might be better described as "cussing") and a threatening shake of Hull's cane in the direction of the offender. In contrast to his contempt for slipshod thinking, Hull encouraged students to disagree with him on theoretical issues. His graduate seminars were suffused with an air of intellectual excitement. He was always willing to assume positions or make statements which stood a good chance of being wrong, if such behavior contributed to a closer examination of the system and if the statements were open to experimental proof or disproof. Of public reactions to his own theorizing as represented by the *Principles*, Hull once wrote the following statement: "In general I have found the hostile examinations much more helpful in the continuing development of the system than the more friendly ones."

His prevailing ill health exerted its effects upon Hull's general outlook. The early attack of typhoid, the subsequent contraction of poliomyelitis, and a coronary attack in 1948 combined to convince him that he would not live long enough to complete the grand program he had laid out for himself. This feeling contributed to his single-minded devotion to research and impatience with any inter-

ference or delay. Toward the end of his life, failing strength limited him to four hours of work each day, and even this could only be accomplished in two two-hour stints. Nevertheless, he persevered and completed the manuscript of his last book, *A Behavior System*, which was published after his death.

Hull utilized the services of many helpers during his research career. One of the most faithful and devoted assistants was Miss Ruth Hayes, who served him for many years during his appointment at Yale. At different times he enlisted the aid of undergraduates, and upon occasion even the janitor was pressed into service as a runner of rats.

Hull never conducted experiments himself. His habitual working attire include a white laboratory coat and a green eyeshade, but he left the actual testing of his theories to volunteer and paid assistants. His interaction with students was close and was not confined to the lecture room or laboratory. He frequently lunched with students and younger faculty members at a small restaurant near the Institute, and these occasions invariably were marked by spirited discussion and argumentation concerning psychological experiments and theories. In later years Hull's poor health deprived him of such contacts, which he valued highly as a source of informed criticism.

In his autobiography, Hull specifically acknowledged his debt to former students and associates. The list which he compiled included Everet F. Patten, St. Clair A. Switzer, Kenneth W. Spence, Neal E. Miller, John Dollard, O. H. Mowrer, Robert T. Ross, Marshall Hall, Donald P. Perkins, Carl I. Hovland, Eleanor J. Gibson, and Harry G. Yamaguchi.

Toward his graduate students Hull did his best to maintain an attitude of meticulous fairness, always taking great care to make public acknowledgment of the original contributions made by others. Quite frequently results of studies inspired and designed by Hull appeared over the name of the student who carried out the actual experimentation. Hull was not an outgoing or particularly warm per-

son, but he commanded the respect of his associates and the affection of those few who knew him intimately.

He tried to keep in touch with his students after they left Yale, and in his outer office was kept a collection of snapshots of former students, their wives, and their children.

He was president of the American Psychological Association in 1935-36. He was a member of the National Academy of Sciences, the American Academy of Arts and Sciences, and the Society of Experimental Psychology. The latter society awarded its cherished Warren Medal to Hull in 1945 with the following citation.

"To Clark L. Hull: For his careful development of a systematic theory of behavior. This theory has stimulated much research and it has been developed in a precise and quantitative form so as to permit predictions which can be tested empirically. The theory thus contains within itself the seeds of its own ultimate verification and of its possible final disproof. A truly unique achievement in the history of psychology to date."

The impact of Professor Hull's work upon psychology cannot be fully estimated at this time, but it has been powerful and widespread. His contributions to aptitude testing and to the study of suggestibility still influence present thinking. His system of behavior theory may well have produced more controversy and provoked more experimental work than any other psychological theory in the twentieth century. The system will not stand unaltered, as he knew it would not, but it will always be an important landmark in the history of theoretical psychology; and those parts of it which withstand the test of time will inevitably influence and perhaps be woven into later formulations by subsequent generations of psychologists.

Hull probably had less interest in his theory as such than in the broader issue of contributing to the solution of social problems. His autobiography closes with the following statement:

"I believe that one of the greatest sources of international conflict and human misery lies ultimately in our prevalent subjectivity. It is bad enough to have religious considerations interfere with the evolu-

tion of science. It is even more surprising and quite as unfortunate to have an international socio-economic system do so. Let us hope that with a sufficiently clear objectivity in our behavioral science these biases and their deplorable sequels will largely disappear. Perhaps the most effective means to that great end will be the accurate and wholly convincing determination of the primary laws of human behavior, together with the scientifically true and unmistakable definition of all critical terms involved. These laws should take the form of quantitative equations readily yielding unambiguous deductions of major behavioral phenomena, both individual and social. Present achievements are small, but the goal at least now seems fairly clear."

KEY TO ABBREVIATIONS

- Amer. J. Psychol.=American Journal of Psychology
 J. Abn. Soc. Psychol.=Journal of Abnormal and Social Psychology
 J. Amer. Statist. Assn.=Journal of the American Statistical Association
 J. Appl. Psychol.=Journal of Applied Psychology
 J. Comp. Physiol. Psychol.=Journal of Comparative and Physiological Psychology
 J. Comp. Psychol.=Journal of Comparative Psychology
 J. Educ. Psychol.=Journal of Educational Psychology
 J. Educ. Res.=Journal of Educational Research
 J. Exper. Psychol.=Journal of Experimental Psychology
 J. Gen. Psychol.=Journal of Genetic Psychology
 J. Philos.=Journal of Philosophy
 Ped. Sem.=Pedagogical Seminary
 Philos. Sci.=Philosophy of Science
 Psychol. Bull.=Psychological Bulletin
 Psychol. Clinic=Psychological Clinic
 Psychol. Rev.=Psychological Review
 Trans. N. Y. Acad. Sci.=Transactions of the New York Academy of Science

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DUNHAM JACKSON

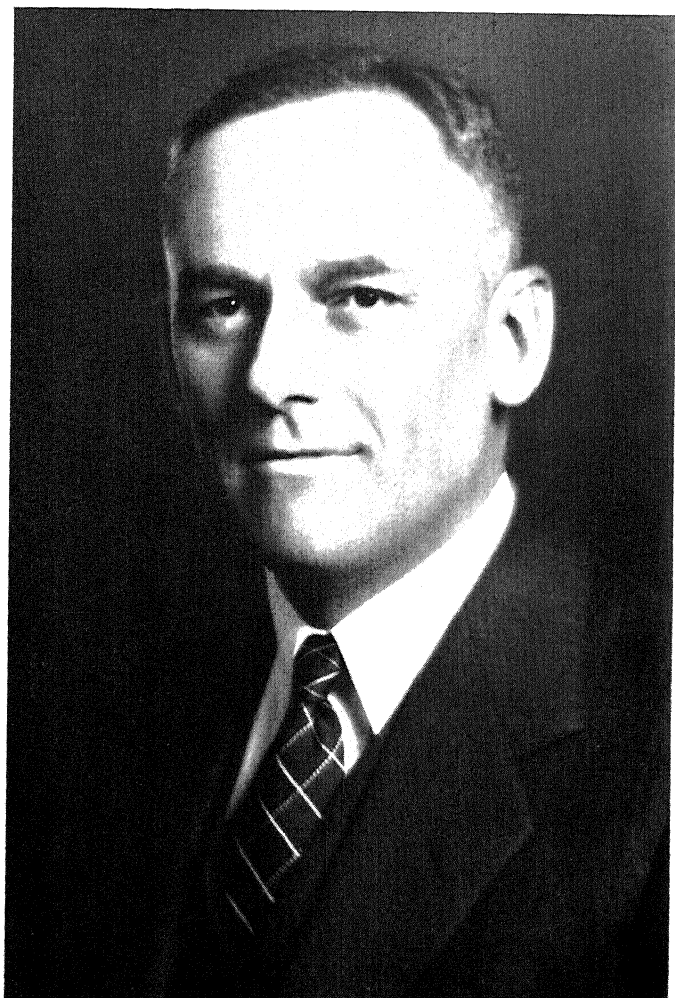
July 24, 1888–November 6, 1946

BY WILLIAM L. HART

MY FIRST MEETING with Dunham Jackson occurred in September, 1916, at Cambridge, Massachusetts, when I arrived to begin an interesting year as a Benjamin Peirce instructor in the Department of Mathematics at Harvard University. At the time, Jackson was an assistant professor in that department. He immediately made me feel as if we were old friends, and this was typical of his reactions to new acquaintances. Later, he and I served together briefly in mathematical work in the Army. Thereafter, starting in 1919, we were colleagues in the Department of Mathematics¹ at the University of Minnesota. Hence, except for an interruption lasting from 1917 to 1919, I had continuous intimate contacts with him from 1916 until his death. In writing this biography, I have no fear of failing to do justice to his mathematical achievements, which will be well documented by the record of his publications and the honors which he received. The main problem of the biography will be to weave into the mathematical record a vivid picture of Jackson's exceptionally warm personality, which endeared him to practically everyone who had contact with him in any capacity.

Before beginning a somewhat chronological account of Jackson's

¹ Meaning, in this memoir, the Department of Mathematics in the College of Science, Literature, and the Arts (SLA). At the University of Minnesota, in 1919 and for many years thereafter, mathematics was taught in the Institute of Technology by its Department of Mathematics and Mechanics. At present, the University has two Departments of Mathematics, one in the Institute and one in SLA, which combine for graduate work.



Winham Jackson

life, it appears appropriate to outline its main features, many of which will be described in more detail later, and to sketch in bold outlines his attitudes and general characteristics. He received the degrees A.B. and A.M., with mathematics as his major field, at Harvard University. His remaining mathematical training was obtained in Europe, and he received his Ph.D. degree at Göttingen University in Germany, with Edmund Landau as the adviser for his dissertation. Jackson was a member of the Department of Mathematics at Harvard from 1911 to 1919, with a leave of absence in 1918-19, to serve as a captain in the Ordnance Department of the United States Army, where he performed mathematical work. For the remainder of his life, from 1919 to 1946, he was a professor in the Department of Mathematics at the University of Minnesota, and hence a large part of this biography will refer to that period. He presented his first scientific paper [1] to the American Mathematical Society (A.M.S.) in 1909 while he was a student working for the degree A.M. His last paper [75] was presented to the A.M.S. posthumously on December 26, 1946. In all, he published 75 mathematical papers involving novelty in the results, or in the methods and organization. He was the author of two books [45 and 67]. Also, he published various articles of general mathematical interest intended for teachers at the secondary or college level, or for the general public. While he was in the Army, he wrote a brief textbook on numerical integration in ballistics in pamphlet form for the Ordnance Department [15]. He was noted for his exceptional ability as an expositor of mathematics in his published work, in lectures, and in teaching at all levels. His high productivity as a scholar becomes more impressive when account is taken of the fact that ill health after the age of fifty-two and death at fifty-eight cut off at least a dozen years of vigorous research by him.

The special abilities of Jackson and his many services to the field of mathematics were appropriately recognized during his lifetime. He was elected to the honor societies Phi Beta Kappa and Sigma Xi while he was a student at Harvard. He was a member of the A.M.S.,

Mathematical Association of America (M.A.A.) (one of its charter members), National Council of Teachers of Mathematics, American Physical Society, American Association for the Advancement of Science (A.A.A.S.), American Academy of Arts and Sciences, and the National Educational Association. For several years, starting while he was a member of the faculty at Harvard, he was a member of various committees on examinations in mathematics for the College Entrance Examinations Board. For an extended period, he was a member of the Committee on the Teaching of Mathematics of the North Central Association of Colleges and Universities. He was a member of the Council of the A.M.S. (1918-20), Vice-President (1921), and a member of the Editorial Committee of the *Transactions of the A.M.S.* (1926-31). He gave the colloquium lectures of the A.M.S. for 1925. He was a member of the Board of Governors of the M.A.A. (1923-29), Vice-President (1924-25), and President (1926). He was awarded the Chauvenet Prize for exposition of the M.A.A. for the period 1932-34. He was a vice-president of the A.A.A.S. (1927), and Secretary of its Section A (Mathematics) for many years. He was a fellow of the American Academy of Arts and Sciences and was elected a member of the National Academy of Sciences in 1935. He always maintained a lively interest in mathematical affairs at the secondary level, and frequently played an active role in joint actions of the fields of college and secondary mathematics.

Jackson was a man of high ideals, extremely unselfish both in ordinary life and in regard to matters of priority in mathematical research. He was very conscious of his responsibilities not only in strictly personal affairs but also with respect to his profession as an educator and his duties as a citizen. He was intensely devoted to the field of mathematics, thought deeply of its place in advancing human welfare, and was fluent in expressing his convictions on such matters. He was reticent in personal matters, and was tolerant and lacking in undesirable curiosity about the opinions and actions of others. An outstanding characteristic was his enjoyment of close associations with people. In particular, his congenial wife and children

were a continuing source of pleasure to him, and he was deeply attached to his father, mother, and sister. It was typical of Jackson that after a few contacts he was likely to call a new acquaintance, including any student, by his given name as a sincere implication of permanent regard. At the University of Minnesota he transmitted this habit to members of his department, so that it became noted for its exceptionally friendly atmosphere, among both students and teachers. As a result of this attitude, his numerous office consultation hours always were heavily attended by students, who probably came as much for the intimate contacts with him as for mathematical assistance. Until his final illness restricted his activities, he was a regular attendant at meetings of the A.M.S., where his cheerful manner and cordiality, in addition to his wide knowledge and active interest in all mathematical programs, made him a great asset. One of his major regrets during his last few years was his inability to attend meetings of the A.M.S. to renew old friendships and to make new friends.

Jackson was apparently in excellent physical shape, and in a vigorous period in his mathematical research, in 1940 at the age of fifty-two, when he experienced a serious heart attack. After his recovery, for about two years, he was able to live a relatively normal life, except that extra precautions had to be exercised concerning physical exertion, and he was restricted as to travel away from home. Then his health began to deteriorate, with a succession of minor heart attacks, and he was able to teach only irregularly. After 1943, he was frequently confined to his room at home or in a hospital, and this was almost continuous for the last eighteen months of his life. Nevertheless, he maintained his scientific activity. During his last year, he gave approval to three² Ph.D. dissertations, the final one just two weeks before his death. His last research paper was finished

² In these cases, he was assisted by Professor Robert H. Cameron, who came to the Department of Mathematics at Minnesota in 1945. Another thesis, originally directed by Jackson, was not wholly complete when he died, and the Ph.D. degree for the student was not awarded until later, with Cameron as the adviser.

only a few months before he died. In spite of his long and trying illness, he maintained creative ability, mathematical contacts with his advanced students, interest in the affairs of the A.M.S., and his normal relations with his family and many friends to the end of his life.

With the preceding sketch as a background, I shall now consider Jackson's life and achievements in more detail.

Dunham Jackson was born on July 24, 1888, son of William Dunham Jackson and Mary Vose Jackson in Bridgewater, Massachusetts. The family was of straight Puritan stock, descended on both sides from passengers on the "Mayflower." His father was a professor of science and mathematics in the Normal School at Bridgewater. This was an institution of high quality emphasizing experimental departures, and finally became a four-year college. Jackson's parents, as well as many relatives, were graduates of the Normal School. The father took added training, but no degree, at the Massachusetts Institute of Technology. Originally he taught botany, geology, physics, chemistry, and astronomy at the Normal School. When it became a college, he taught physics and mathematics through the level of elementary calculus.

The early life of Jackson had an intelligently religious orientation. The members of his family were Congregationalists.³ His father was a deacon and treasurer of the church and, for many years, was superintendent of the Sunday School. The tone of the household was consistent with this background. When Jackson was in his teens, the minister of the church was a man of exceptional talents, brilliant, widely read, and alive to modern currents of religious thought. He was very stimulating to the young people of his congregation and gave them a firm, enlightened religious foundation. Jackson maintained a corresponding attitude with respect to religion throughout his life. His high ideals were in harmony with these early influences in his home and community.

³ I am indebted to Jackson's sister, Dr. Elizabeth Jackson, Associate Professor of English at the University of Minnesota, and to Jackson's diary, for many of the details in this biography.

The parents of Jackson undoubtedly noticed early in his life that he had exceptional intelligence, as well as an unusual share of excellent characteristics. As soon as Jackson was of walking age, he became his father's companion on long hikes and his father's interest in science made these excursions essentially field trips for the study of botany, zoology, and geology. Thus, very early, Jackson acquired from his father a vast store of miscellaneous information and habits of observation that he never outgrew. At an early stage, he started reading the science textbooks in his father's library. Once, the family doctor became concerned over Jackson's health and prescribed a week's vacation from astronomy, chemistry, and physics. Jackson was heartbroken and pleaded, "Can't I even read astronomy? That isn't so exciting." During his years in high school, he showed unusual aptitude and liking for languages and literature. From both his father and his mother, he inherited a love of poetry and the habit of learning it by heart. He had an exceptional teacher of English in high school who motivated him to take the examination for exemption from freshman English when he entered Harvard University. He was less fortunate in his other teachers in high school. At that time, he took great pleasure in his three foreign languages—Latin, Greek, and German. He enjoyed learning Latin and German poetry by heart, and was fond of teaching it to his young sister when they worked together at household chores. As a consequence of the Puritan prohibition of study on Sunday, he once developed the bright idea of utilizing his father's collection of Bibles in foreign languages, and read the gospels in Greek as a legally religious and also intellectually advantageous Sabbath diversion. In later life he frequently read Greek literature for relaxation.

It would be a mistake to imagine that Jackson spent a wholly or even primarily bookish childhood. It is a tribute to the intelligence of his parents that his intellectual interests, up to the time he went to college, were kept in proper balance in his activities. He shared all the regular amusements of boys of his age—baseball, tennis, skating, cycling, swimming, sailing, and hiking. He maintained these

athletic interests throughout his life. Thus, when he became a graduate student at Göttingen, he regularly played baseball, hockey, and tennis, and took long hiking and bicycle trips around the neighborhood. Even though an attack of polio (to be discussed later) curtailed his physical activities in later life, he continued to engage in all possible forms of his favorite athletic diversions. In fact, up to the time of his heart attack at the age of fifty-two, he gave evidence of a constant excess of physical as well as mental energy. His fondness for participation in sports carried over to a strong interest as a spectator at sports events. Thus, in his later life at the University of Minnesota, he rarely missed athletic contests of University teams in any of the sports. At any athletic competition, he always managed to become a strenuous partisan and enjoyed such affairs keenly.

Jackson was barely sixteen years of age when he entered Harvard University. His record as an undergraduate was unusually distinguished, not only in his field of concentration, mathematics, and in the related fields of astronomy, chemistry, and physics, but also in course work in various ancient and modern languages. As a sophomore, he received the Wendell Scholarship as the leading student of his year. He had highest second year honors in both mathematics and the classics. During his senior year, he received the Palfrey Exhibition and the Richard Augustine Gambrell Scholarship in recognition of his achievement as first scholar of the senior class. He received the A.B. degree *summa cum laude* in 1908, with distinction in mathematics. His record in the Graduate School, completed by the award of the degree A.M. in 1909, was equally distinguished. During 1908-09 he was one of seven students receiving George C. Shattuck Fellowships for "persons of superior merit in the study of mathematics or various languages." He also held an assistantship in astronomy. In 1909, after obtaining the degree A.M., he received the Rogers Fellowship (1909-10) and the Edward William Hopper Fellowship (1910-11) from the Graduate School at Harvard University, for study toward the Ph.D. degree at Göttingen.

It is interesting to note that Jackson's first published research in

mathematics [1], in the field of algebra, was written shortly after he received the degree A.B. His inspiration for this work undoubtedly came from his courses in algebra under Professor Maxime Bôcher, and from exceptionally close contacts with him outside of the classroom. These contacts continued in correspondence all during Jackson's absence in Europe. Thus, we find from his diary that he and Bôcher exchanged frequent letters, sometimes as often as once a week over long periods. The content of these letters is not disclosed in the diary, but undoubtedly they dealt largely with mathematics of joint interest, or with advice to Jackson concerning his mathematical life in Europe. In all, it is clear that Bôcher and Jackson were close friends, and that Bôcher exerted great influence on him in his early life as a mathematician.

Jackson arrived in Göttingen in September, 1909, shortly after he was twenty-one years old. The ensuing two years were an intensely interesting period in his life. He started a diary on his twenty-first birthday, and carried it forward meticulously, with rarely a day of omission for about five years. All specific details which I shall give concerning his life during his student days in Europe will be based on information presented by him in this diary.⁴ He started it with the following statement: "I shall keep this record for my own convenience and pleasure and not for the purpose of constructing a model diary. If I sometimes write what I think, as well as what I do, it will be for the sake of comparison and not because the thoughts are of any value in themselves. I intend to tell the truth, and nothing but the truth, but not the whole truth." In the preceding statements, he distinctly was in error in certain respects. The diary, consisting of hundreds of pages, indeed is a model for such a composition, if it is to give a clear picture of the author. In addition to a listing of day-by-day events, Jackson's diary frequently contains a fascinating record of his most important thoughts. In reading them we gain a clear impression of the character and of the workings of the mind of a

⁴ The diary was placed at my disposal recently by Jackson's sister.

brilliant young scholar, his philosophy about life in general, his evaluation of the psychology of his major field, and his reactions to problems in the field of education. Jackson also was wrong in estimating that his thoughts, as transmitted to his diary, were of no value. For instance, even at this late date, I believe that many of his philosophical musings about mathematics and its pedagogy could well form the basic themes for extended discussion. He adhered to his decision that he would "not tell the whole truth." He was very reticent about personal affairs in his diary, just as in later life he maintained reticence in such matters even in contact with his most intimate friends. Hence, a reader of the diary does not feel that he is intruding on the privacy of its author. It appears appropriate to quote a few passages from the diary, illustrating its scope outside of mere factual information. Also, the quotations give some indication of the unusual maturity of Jackson at this time, his philosophical introspective nature, his serious attitude about religion, and his well-organized opinions about the psychology of learning and teaching mathematics.

In regard to church hymns, after attendance at church one day in Germany: "Most of the words were written by men who have been dead for one century, if not two. I have tried to make out what common characteristic it is that seems to make them all so remote from practical life. . . . The thing that is lacking in the hymns is perhaps any suggestion of our intelligently doing anything. We can only wonder, and worship, and pray for guidance. Precisely this way of thinking has accomplished great things."

Concerning college education: "I have been reading President Lowell's (Harvard) inaugural address. I went on to think about the problem of improving college education, as I do occasionally, and was struck by an idea which impressed me very much. (I must wait now for it to cool off.) The idea is to add to the present general courses for average students, and special courses for good students, a few general courses for good students, and make them open only to distinguished seniors."

Comment on address of President Nichols of Dartmouth, where it was stated that we need special knowledge in college teachers but not specialized men: "The question is, is it possible for a man to get the specialized knowledge to the extent that is necessary without himself suffering some specialization? Some men, surely, are successful in this, but I suspect that it is easier in some fields than in mathematics." (Any qualified observer would agree that Jackson himself furnished a good example of a specialist in knowledge who did *not* become a specialized or unnatural man.)

Mathematical teaching at advanced levels: "What is the purpose of a higher course in mathematics? To teach facts or methods? Should a course be simply a succession of theorems with proofs? Or, is it undesirable to try to prove everything? Does a mathematician read and make absolutely clear to himself all the work on which his own original work is based, so that he can assert, of his own knowledge, that the preceding work is true? Or, does he leave something to the accuracy of his predecessor? This is a point on which information ought to be gained by inquiry. Is the purpose of proofs in a higher course to teach methods which will be useful to the pupil or to assure him that every point is based on a rigorous foundation? If the answer to these questions is different for different courses or for different parts of a course, I think it would be well for the teacher to have in mind just what point of view he is adopting at each stage."

A basic attitude for any school: "Every school ought to be a normal school teaching the pupils to teach themselves, actually telling them how to do it, how to observe the success of various methods, not just giving them the example and hoping they will observe." "I have been thinking again that a university ought to teach students to use their minds better than it does. . . . no instruction is given, not much anyway, about *how* to work."

The text-book level of presentation of mathematics to immature students: "Looked at Kowalewski's Calculus. The theoretical work with which it begins would be out of the question in America. The American student wouldn't be able to understand it. I don't believe

the German is either. But, the German learns it obediently for the time being, and comes to an understanding of it by and by. The American wouldn't do that. In the first place, he wouldn't keep on studying something he didn't understand. And, in the second place, with the best of intentions he wouldn't be able to get any grasp of it, even to the extent of remembering it, if he didn't understand it. That is the way he is made, judging by me."

Mathematics in general education: "I have been wondering whether it would be possible to give a course of lectures on descriptive mathematics so that seniors in college who had never heard of a sine when they began could tell an integral equation when they saw it at the end of the year. It would be hard to do it, but I am not sure it couldn't be done. I should want to emphasize the fact in every lecture that mathematics is a science of finding out what is so and what isn't and not a juggling with formulae according to mysterious and irresponsible rules."

How to work at mathematics: "It often happens that the ideas that are worth most come when I am just on the point of stopping work. I plug away for a while and don't get anywhere, and make up my mind to stop, and then as my mind wanders back involuntarily over what I have been pondering, some relation that I had overlooked takes form as if by itself."

In spite of numerous philosophical touches as just indicated, the diary does not make heavy reading. Thus, Jackson frequently included his own humorous interpretations of events, or amusing anecdotes or jokes of companions. His hearty sense of humor is well shown by such features, and they prepare one to expect the fondness which he showed in later life for jolly spinning of limericks or other verbal amusements with friends. The following lighter touches in the diary bear out the preceding remarks:

"When you have got a bad cold, and it is dark and rainy, and you have lost your umbrella, and can't make head nor tail out of what you are trying to do, you have something to look forward to when circumstances change."

“ . . . Hurwitz’s pun last night: ‘What is the shape of a kiss?—It’s ellip-tical.’ ”

“A Berlin joke: He fell into the river and was drowned. How did that happen? I thought he could swim. Answer: But it is verboten to swim in the river.”

“I have been trying to prove the uniform convergence of a series. It doesn’t converge worth a pfennig!”

Several quotations ascribed to the mathematician Felix Klein show that he could be quite humorous and informal in the classroom, and that Jackson enjoyed this feature. The following are Kleinisms:

“You can make a hyperbolic geometry for yourself by twisting your neck.”

“Die Mathematik ist die Wissenschaft der dinge die selbstverständlich sind” (quoted by Klein from Jacobi).

Jackson arrived at Göttingen in a golden age of that mathematical center, and made it his headquarters from 1909 to 1911. Its staff during this period included the mathematical giants David Hilbert, Felix Klein, Edmund Landau, and Ernst Zermelo, as well as others who were not so famous. In addition to attending lectures under members of the staff, Jackson also took part in seminars and attended the Mathematische Gesellschaft, at which the speakers were local mathematicians or, occasionally, prominent visitors. Jackson immediately received cordial attention from members of the mathematical staff. He credited some of the special interest in him to the fact that Bôcher had mentioned his name in the preface of the German translation of Bôcher’s text on higher algebra, for a suggestion by Jackson which had come up in a course under Bôcher at Harvard. Bôcher’s book was of interest at that time in Göttingen. For a few months in 1911, Jackson attended the University of Bonn, where he heard lectures by Felix Hausdorff and Aduard Study. Also, Jackson spent a few weeks of 1911 in Paris, where he met and attended lectures by Emile Picard, Edouard Goursat, and Jacques Hadamard. During Jackson’s stay in Europe, we find from items in his diary

that he had personal contacts or correspondence with practically all of the famous German and French mathematicians of that period. Also, he initiated friendships with many young European mathematicians who became famous later. Jackson was fascinated by these contacts and, as might be expected, at times was somewhat awed by lectures given by various famous men. As a matter of general mathematical history, it is interesting to learn from Jackson's diary that he met as students in Europe many of his American contemporaries in mathematics, who later became leaders in the A.M.S.

At Göttingen, Jackson selected Landau as the adviser for his doctoral dissertation. I infer that advice from Bôcher partly dictated this choice, because Jackson's diary contains the following statement, during a visit made to Göttingen by Bôcher in January, 1910: "As a result of a talk of Professor Bôcher with Professor Landau, I went to see the latter this afternoon and got a list of possible subjects for a dissertation." Jackson's choice of a subject for his thesis permanently channeled his main future research into the field of approximation theory and orthogonal functions, which he enriched by many new results and outstanding expositions of fundamental content. In the preface of the book containing his colloquium lectures [45], delivered to the A.M.S. in 1925, he made the following remarks concerning his choice of a thesis topic: "Guided partly by natural inclinations, perhaps, and partly by recollection of a course on methods of approximation which I had taken with Professor Bôcher a few years earlier, I committed myself to one of the topics which Landau had proposed (for a thesis), an investigation of the degree of approximation with which a given continuous function can be represented by a polynomial of given degree."⁵ When I reported my choice he said meditatively . . . *Das ist ein schönes Thema, ich beneide Sie um das Thema . . . Nein, ich beneide Sie nicht, aber es ist ein wunderschönes Thema.*"

⁵ In considering this topic, Jackson enlarged it to include approximation by trigonometric sums as well as by polynomials.

The primary result of his thesis was as follows:

There exists an absolute constant K_1 with the following property: if $f(x)$ has the period 2π and satisfies a Lipschitz condition, $|f(x_1) - f(x_2)| \leq \lambda |x_1 - x_2|$, then, for every positive integer n , there exists a trigonometric sum $T_n(x)$ of order n at most so that

$$(1) \quad |f(x) - T_n(x)| \leq K_1 \lambda / n$$

for all values of x , and thus $\max |f(x) - T_n(x)| = O(1/n)$.

In his demonstration of this result, $T_n(x)$ was explicitly exhibited. Under similar hypotheses on a function $f(x)$ defined on an interval (a, b) , omitting reference to periodicity, he exhibited polynomials $P_n(x)$ of degree n at most so that $|f(x) - P_n(x)| \leq G_1 \lambda l / n$, where $l = b - a$ and G_1 is an absolute constant. Moreover, if $T_n(x)$ now represents the trigonometric sum of order n at most for which $\max |f(x) - T_n(x)| \equiv \phi(n)$ has its least value, Jackson proved the important result that, under his hypotheses, in general $\phi(n)$ is not of lower order than $1/n$, with a similar result for the polynomial case. If $f(x)$ possesses an $(h-1)$ th derivative satisfying a Lipschitz condition, Jackson obtained $K_h \lambda / n^h$ in place of the right-hand side of (1), and a parallel result for polynomials. He proved similar theorems for a function $f(x, y)$ with respect to approximation by polynomials $P_n(x, y)$. The key detail in Jackson's thesis was an expression for $f(x)$ by means of a new type of singular integral, probably suggested by those which had been employed previously in proofs of Weierstrass's theorem about polynomial approximation. Jackson's thesis answered a prize question which had been proposed earlier by the Göttingen faculty: "Would it be possible to improve on results (on approximation by polynomials) previously obtained by de la Vallée-Poussin and Lebesgue?" In a complimentary statement awarding Jackson the prize, we find the remark: "The author . . . has enriched the science with valuable results, all in competition with mathematicians of the first rank."

It is no reflection on Jackson's later achievements to remark that his thesis was one of his most important pieces of mathematical re-

search. The fact that his results improved on those of very famous mathematicians gives sufficient assurance of the quality of his thesis, and Landau was amply justified when he labeled its topic "ein wunderschönes Thema." Jackson thus had the rare good fortune to produce, in his first major effort, results which were fundamental for the development of a major field. Although much of his future research was more polished and more difficult than that in his thesis, only a few of his later results (as is usual in research) had the peculiarly basic importance of certain theorems in his dissertation.

The outstanding achievements of Jackson as a student in Europe become more impressive when we take note of the fact that, about halfway through this period, he experienced an illness leaving disturbing permanent effects, which could have injured the spirit of a man with less courage and intellectual balance. At some time during the spring of 1910, he suffered an attack of polio, which fortunately was only moderately severe. As evidence of Jackson's cheerful disposition, there is not a single omission in the daily entries of his diary, and there is no explicit mention of the illness, to indicate its exact date of occurrence. At that time, as in later life, he refused to complain about ill health, even in the privacy of his diary. The illness left him with a permanent abnormality in the muscles controlling one foot and caused considerable lameness for the rest of his life. Before the polio attack, his athletic interests at Göttingen had covered the whole gamut of usual games, and diversions such as bicycling and hiking. Suddenly, none of these activities were permissible for him, although eventually he was able to resume participation moderately in some sports. With typical reticence, he avoided giving details of his illness in his diary. Also, with a similar attitude, he kept immediate information about the matter from the members of his family. Their first knowledge of it came fifteen months later when he returned to Bridgewater. His innate cheerfulness and good morale is shown by the following entry in his diary several months after the polio attack: "Rode my bicycle in the garden. Doesn't go very well, but I am glad to be able to do it at all." As far as one can tell from

his diary, Jackson allowed the illness to keep him from lectures for only a few days. If anything, one senses increased vigor at this time in his devotion to his mathematical activities.

On the nonmathematical side, Jackson enjoyed his two years in Europe to the fullest extent. He made friends very easily, in Europe as in America, and deeply enjoyed his contacts with them, in person or through correspondence. Thus, his day-by-day record in his diary of letters sent and received gives evidence of exceptionally heavy correspondence, where the list of correspondents at first was composed only of American acquaintances and gradually was enlarged by the addition of many people whom he met in Europe. His diary gives extensive accounts of his social contacts in Göttingen, and later in Bonn and in France, with both the faculty and students of universities which he attended. One obtains a picture of frequent extracurricular activities in the colony of American students, from all fields, at Göttingen. We learn that, in Jackson's second year in Europe, he became "Patriarch" (the administrator) of the colony. During his vacations, he travelled extensively in Germany, and less widely in France and Italy.

In his accounts of his travels, we find evidence of the breadth of his interests. He took great pleasure in viewing beautiful scenery, foreign cities, and works of art. The diary contains elaborate descriptions of features of interest. He philosophizes about art and sculpture, about their appreciation, and about the people whom he observed. All during his stay in Europe, and particularly on his travels, he took every possible opportunity to attend musical performances. Throughout his life, he maintained his interest in the fine arts and the theater. As evidence that he was normal in his reactions and by no means was a "specialized man," he even extended these outside interests to movies, and had many favorite actors and actresses in them.

Jackson started his Göttingen dissertation in the spring of 1910, and finished the job in April, 1911. He passed his doctoral examination and received his degree *magna cum laude* on July 12, 1911. His thesis was published separately as a "Gekrönte Preisschrift" of 98

pages, by Göttingen University in 1911, and never appeared in exactly its original form in any mathematical periodical. Later he published refined and simplified forms of the main results of the thesis in the *Transactions* of the A.M.S. He left Europe for the United States in the summer of 1911.

Early in 1911, Jackson was offered an instructorship in the Department of Mathematics at Harvard, and he returned there to start the school year in September, 1911. In the inspiring mathematical atmosphere of Harvard, his closest contacts were with Maxime Bôcher, George D. Birkhoff, Julian Coolidge, William F. Osgood and, later, the young geometer Gabriel Green, whose untimely death in the influenza epidemic of 1917-18 cut short a brilliant career. Jackson's research from 1911 to 1918 consisted of progress in his own major field [3-8, and 13]; a pair of articles relating to differential equations [9 and 12], and three papers dealing with the theory of functions of complex variables, where we sense the influence of Osgood [10, 11, and 14]. During this period, Jackson also directed the research of his first two doctoral candidates. I was a close companion of Jackson at Harvard during 1916-17; at that time, he and Green were the youngest permanent members of the Department of Mathematics at Harvard. Although the whole department was a great inspiration to a young mathematician, Jackson and Green were the foci of the mathematical life and much of the social life of the younger men outside of their lectures and seminars. In a remarkable degree, both Jackson and Green possessed, first, natural ability to establish cordial friendships with their associates and, second, unusual power to encourage and assist young men in performing mathematical research. A request for mathematical information from one of them never was met with an attitude leading the questioner to be embarrassed over his ignorance. The request always was treated as if it were an indication of special understanding and commendable intellectual honesty. This attitude of Jackson in disseminating mathematical knowledge was one of his most valuable char-

acteristics as a teacher, as a director of dissertations, and later as a mathematical leader in his department at Minnesota.

In April of 1917, the First World War broke up the community of graduate students and young mathematicians at Harvard. Many of us left immediately for officers' training camps. About one year later, Jackson was commissioned ⁶ as a captain in the Ordnance Department and was assigned to the Ballistic Unit of the Technical Staff of the Ordnance Department in Washington, D. C., directed by the mathematical astronomer Forest Ray Moulton (then Major, Ordnance Department). At various times, many mathematicians ⁷ were attached to the unit, either as civilian employees or as Army personnel. Although the routine calculation of range tables for the artillery services was a major function of the Unit, its work was carried on in a scholarly atmosphere, as far as was possible under wartime conditions. From 1918 to 1920 ⁸ various advances were made in ballistic theory and actual artillery practice through cooperative actions of the members of the Washington Unit and members in a corresponding Unit at the Aberdeen Proving Grounds in Maryland, under the direction of Oswald Veblen (then Major, Ordnance Department). At Washington, Jackson became one of the most useful members of Moulton's staff. As part of the work, Jackson prepared an excellent pamphlet [15], suitable as a brief textbook, presenting fundamentals of the small arc method of numerical integration for differential equations as developed by the Ballistic Unit for the com-

⁶ By special action, his physical handicap was disregarded, in view of the noncombat nature of his intended activities in the Army.

⁷ In particular, J. W. Alexander, A. A. Bennett, Arnold Dresden, Lester R. Ford, Phillip Franklin (before he had had graduate work), H. T. Gronwall, William L. Hart, W. D. MacMillan, William E. Milne, and J. F. Ritt.

⁸ The Ballistic Unit was kept active between the First and Second World Wars by the devoted service of the physicist Robert Kent, who preceded Moulton in the work at Washington, and who remained as a civilian employee of the Ordnance Department after the end of the First World War. In the Second World War, the work expanded tremendously to become the Ballistic Research Laboratory of the Army, at Aberdeen, with Kent as the director.

putation of trajectories in ballistics. Jackson resigned his Army commission in 1919.

During the spring of 1919, Jackson was offered an appointment as Professor of Mathematics in the College of Science, Literature, and the Arts at the University of Minnesota. Undoubtedly, even with a large increase in academic rank, it was hard for him to think of leaving Harvard University, where his future progress seemed assured. At this time, I was working with him in Washington and also was considering an offer of an assistant professorship at Minnesota. With typical reticence about his own affairs and, perhaps, with the desire to avoid influencing my decision, he never acquainted me with the details of his thoughts as he considered the offer from Minnesota. However, I suspect that he was swayed largely by a description of future possibilities in the position as pictured by the brilliant President Marion LeRoy Burton ⁹ of the University of Minnesota, and by the frank, effective salesmanship of William H. Bussey, then chairman of the Department of Mathematics at Minnesota. It was admitted that, on arriving at Minnesota, Jackson would find essentially negligible graduate work in mathematics. But he had assurance that there would be a large potential demand for advanced mathematics by itself and also in cooperation with the physical sciences and technical departments. In 1919, the University of Minnesota was starting a period of vigorous growth, at both the undergraduate and graduate levels, which has continued unabated to the present date. I am sure that Jackson came to Minnesota because he visualized a golden opportunity to plan and develop advanced mathematics almost from scratch, in a well-situated university which was young and virile academically, with an explosive potential for future expansion. He was promised freedom from routine administration and a free hand in planning graduate work if he came. Both Jackson and I accepted the offers of positions at Minnesota. We arrived there in September, 1919, and were colleagues until Jackson's death in 1946. I am certain

⁹ Later president of the University of Michigan, where he died after very brief service.

that he never regretted this move. He soon took on the reactions of a native Minnesotan in his pride in the state, and in the intelligence of its citizens in demanding and obtaining a state university of the best quality, immune to politics, regardless of the high cost in comparison with state resources. Also he had great respect for the human qualities of the typical student at Minnesota and his potentiality for intellectual development.

On June 20, 1918, Jackson married Harriet Spratt Hulley, whom he had met in 1917, while she was a graduate student in English at Radcliffe College. Her father was president of John B. Stetson University. The Jacksons had two daughters, Anne Hulley (Mrs. William F. Byess) and Mary Eloise (Mrs. William J. Thorpe). Jackson and his family quickly made a place for themselves in the friendly environment at Minnesota. He frequently became vocal in expressing his satisfaction with the cordial social contacts which were typical of the relations between members of both the academic and non-academic staffs at the University. Also, he felt that the general situation in the state of Minnesota, and in Minneapolis in particular, was ideal for living and for rearing a family. At a later date, his appreciation of living and working conditions at Minnesota led him to decline several very enticing offers of positions elsewhere. Jackson was in the habit of taking frequent brief trips by himself to small towns in Minnesota, especially when he felt the need for relaxed concentration on some piece of research. By the end of his life, there were few Minnesota towns possessed of a reasonably decent hotel, and accessible by rail, which Jackson had not visited. On any one of these excursions, it was his habit to hike over the neighboring countryside, while letting his thoughts crystallize in regard to whatever problem was uppermost in his research. In all, Jackson and his family led a well adjusted and very happy life at Minnesota, with ample cultural advantages, innumerable friends for social contacts, and very congenial associates for Jackson in his work in his department and in the University at large. As a minor light observation, Jackson particularly enjoyed the fact that his home was sufficiently

near a city fire engine station that he could hear the engine sirens when the firemen went out on a call, and thus have the opportunity to pursue them to the fire, which he was likely to do at almost any time of the day or night.

In accordance with early agreements, Jackson was kept free of administrative duties at Minnesota. However, although the Department of Mathematics had a succession of chairmen¹⁰ who commanded substantial University respect, the Department and the University in general recognized Jackson's unique claim to certain phases of mathematical leadership. Also, it was quickly learned on the campus that he had an unusually sound viewpoint about matters of general educational policy. Hence he was frequently sought as a member of committees dealing with important questions at both the undergraduate and graduate levels. He had considerable influence on the development of matters affecting fields related to mathematics. In all, it is no exaggeration to state that, aside from Jackson's activities as a scholar and teacher, he was one of the most valuable members of the faculty at the University of Minnesota.

The Department of Mathematics at Minnesota, under its able chairman Bussey (in 1919) was proud of the excellence of its teaching. Jackson enhanced this reputation both by his teaching and by his indirect influence on the whole department. He had a strong conviction, illustrated by his own activities, that excellent teaching at both the undergraduate and graduate levels is a prime duty, and should yield great satisfaction for any member of a university department of mathematics. In his contacts with younger members of the Department, and with graduate students, he implicitly emphasized ability to communicate and scholarship as companion characteristics which should be cultivated by a member or prospective member of a college faculty. His manner in teaching was informal and friendly, but strictly businesslike. He gave dignity to undergraduate instruction by teaching courses at low levels, particularly

¹⁰ During Jackson's lifetime at Minnesota, the chairmen were William H. Bussey, William L. Hart (twice), and Raymond W. Brink (twice), for the longest period.

elementary calculus, whenever possible. In fact, he gladly would have taught this course every year. At a time when I was chairman of the department, I urged him to teach the course only every third year. I recall his suggestion that he would be glad to teach the course as an extra load, in addition to his popular graduate courses, but I convinced him that this would be a poor policy. His success in the teaching of both undergraduates and graduates was public knowledge on the campus. He took great pleasure in personal contacts with young people, particularly undergraduates, and enjoyed having the opportunity to influence them scientifically at an early age. It was invariably the case that his students thought of him as a sincere friend as well as a skillful and inspiring teacher. It is not surprising then to find that an unusually large percentage of undergraduates who had Jackson as a teacher in elementary courses went on to graduate work in mathematics, the physical sciences, or statistics.

In regard to advanced courses, Jackson had the conviction that, in cooperating with other fields such as the physical sciences, it was the duty of the mathematician to transmit as much knowledge as possible on the basis of minimum prerequisites. Hence, it was his custom to develop courses where the prerequisite was merely a first course in calculus, but where the content as presented went deeply into fields of great usefulness. His most outstanding course of this kind for students of the physical sciences was named "Topics in Analysis," and covered essentially the content in his *Carus Mathematical Monograph* [67]. In its preface he remarks in connection with the low prerequisite, elementary calculus, for appreciating most of the book: "Under the circumstances, rigor in the sense of literal completeness of statement has been out of the question. It is hoped, however, that the reader who is familiar with the methods of rigorous analysis will be able to read between the lines the requisite supplementary specifications, and will find that what has actually been said is entirely accurate in the light of such interpretations." Thus Jackson aimed to tell the truth mathematically but not to make the reader unhappy by giving rigorous details beyond his comprehen-

sion. A further aim was to give the reader of low background a feeling of rigor, and confidence in the soundness of the reasoning. The content of this book for years formed a very popular course in advanced mathematics for students in pure mathematics as well as in the physical sciences. I quote, essentially, a statement made to me by an internationally famous physicist who took the course from Jackson: "It was the most valuable course which I ever had in mathematics. The content came relatively painlessly. It seemed that, with little apparent effort on the part of the students, large amounts of very important mathematics were absorbed. He seemed to make all of it appear easy." This reaction was a common one among students of Jackson. In lecturing, he never seemed to say a useless word. Without any indication of hurry on his part, it suddenly would be realized by the students that he had covered a wide expanse of territory with complete clarity.

One of Jackson's outstanding contributions at Minnesota was his role in the development of statistics throughout the University. In his research on orthogonal polynomials, their use in connection with expansions of statistical frequency functions brought him in touch with certain phases of statistical theory. Also, at the time, he was receiving numerous requests for mathematical aid from faculty members in various fields of applied statistics on the campus. Hence it was natural for him to develop interest in statistics. His publications in this field commenced in 1923 and extended over a period of fourteen years. His papers show evidence that he was giving the foundations of statistics close scrutiny, and was endeavoring to clarify various points which at the time were somewhat obscure mathematically. Hence, his publications on statistical topics [24, 28, 30, 32, 34, 37, 38, 45 (Chapter VI), and 56] involve only a few definitely new results, and are relatively of expository type. His outstanding exposition, "Mathematical Principles in the Theory of Small Samples" [56], consisted of the content of a lecture presented by him at a joint session of the A.M.S., the American Statistical Association, the Econometric Society, and Sections A and K of the A.A.A.S. At the time,

this expository article was unique in clarifying a most important segment in the mathematical foundations of the field. His continued major activity in approximation theory apparently prevented him from performing more fundamental research in statistics. However, his publications in the field, and the nature of a course developed by him at Minnesota, make it evident that he was well along on the highway of modern mathematical statistics. Shortly before his illness at the age of fifty-two, he remarked to me that he hoped to give major attention to developments in statistics which he had in mind, as soon as his well defined program in his main field neared completion. His illness prevented such action. Hence, as his lasting products in statistics, we must point mainly to a course which he introduced at Minnesota, the outstanding record of alumni of that course, and his general influence on statistical development on the campus.

In the school year 1924-25, Jackson introduced a course entitled "The Mathematical Theory of Statistics," three hours per week for a year, at the advanced undergraduate or elementary graduate level, at a time when few courses of this type were available in American universities. He specified the prerequisite for the course as merely elementary calculus, with his usual desire for minimum prerequisites. He always presented the course purely by lectures. It was not modern by present-day standards but, I believe, had more of the spirit of modern statistics than any text of the proper level which was available at the time. Jackson last taught the course (only for part of the year) in 1944-45. Up to this time, the course had been given occasionally by two other members of the Department of Mathematics besides Jackson. The impact of this course on graduate training at Minnesota can be judged in part by the following facts about students who took the course (all except two were taught by Jackson) early in its history. Ten of them ¹¹ have been or are heads of the instruction in bio-

¹¹ It is interesting to note that one of the leading modern textbooks on mathematical statistics, as presented on the basis of elementary calculus as a prerequisite, was written by one of these students: Paul G. Hoel, *Introduction to Mathematical Statistics* (1947, 1954), John Wiley and Sons, Inc. Hoel received his Ph.D. at Minnesota with mathematics as his major, and Jackson as the adviser for the doctoral dissertation (see items for 1933 in the list of dissertations directed by Jackson).

statistics or in statistics for a department of mathematics, or for a college of business administration, in major American universities; one is a world-famous statistician in the field of sociology; another is a noted statistician in the field of psychology. Approximately a dozen other students from the course went on to the Ph.D. degree in some field of applied statistics. Also, when Jackson started the course, practically all of the prominent teachers in fields of applied statistics at Minnesota took the opportunity to audit the course, or otherwise make contact with its content.

The course which we have just discussed arose very opportunely for the University of Minnesota. At approximately the same time, and with added vigor due to the existence of Jackson's course and his availability as a statistical adviser, several significant developments in applied statistics took form in the University. A strong group in biostatistics arose under the influence of the eminent botanist and biometrician, James Arthur Harris (deceased), head of the Department of Botany. He was instrumental in bringing into being a Division of Biostatistics in the Department of Botany; this Division was transferred later to its present location in the Medical School, where now the Division is very large. The first head of the Division was Alan E. Treloar, who was well-informed as to the content of Jackson's course. From the beginning, due to Jackson, and continuing down to the present date, close collaboration has existed between the Division of Biostatistics and the Department of Mathematics. In the School of Business Administration, an exceptionally strong and at that time unique undergraduate major in statistics was created by the economist Bruce D. Mudgett. This major involved the requirements of elementary calculus and Jackson's course in mathematical statistics. Substantial courses in statistics were initiated in the graduate offerings of the Department of Educational Psychology in the College of Education, under the direction of the educational psychologist and statistician Palmer O. Johnson. Statistical work on a high plane was fostered at advanced levels in the College of Agriculture by Forest R. Immer (deceased), the noted agricultural statistician,

who audited Jackson's course, and by the economist Holbrook Working.

In the Department of Mathematics, the wide utility of Jackson's course in statistics (at first given biennially) finally led to its being offered annually, with ever-increasing registration. This in turn has fostered the development of more advanced courses in statistics and in the field of probability, where the Department now exhibits particular strength.

An analysis of the research of Jackson naturally involves simultaneous scrutiny of the research directed by him. Hence, following the appended chronological list of his publications, I have listed the doctoral dissertations which he directed. He was the adviser for two students obtaining the Ph.D. degree at Harvard and nineteen at Minnesota. It will be observed that the topics of the theses are closely related to Jackson's research at corresponding periods.

The mathematical publications¹² of Jackson may be discussed in three categories: (*a*) articles on statistical topics; (*b*) publications in his major field of approximation theory and orthogonal functions; (*c*) other publications. Ample remarks have been made about category (*a*). In category (*c*), I have already mentioned a few publications produced while he was at Harvard or in Europe. In addition, his papers [21, 23, and 31], should be included in (*c*); their titles are sufficiently explanatory. His most significant research occurs in category (*b*), and can be grouped roughly as follows:

1. *His doctoral dissertation, already discussed, and closely related outgrowths of this work*, involving part of his research from 1911 to 1921. In the later stages of this work, his emphasis was on convergence properties of trigonometric and polynomial approximations to a given function $f(x)$. Synthesized general results of this nature are found in a paper [20].

2. *Research during 1921-25 on approximation in the sense of least*

¹² For a more detailed analysis of the publications, see the biographical sketch "Dunham Jackson, 1888-1946," by William L. Hart, *Bulletin of the A.M.S.*, 54(1948): 847-60.

m th powers, $0 < m$, with a non-negative weight function, ρ , introduced to give a measure of approximation $h_{n,m}$ to a given function $f(x)$, say on an interval $0 \leq x \leq 2\pi$, where

$$h_{n,m} = \int_0^{2\pi} \rho(x) |f(x) - T_{n,m}(x)|^m dx,$$

and $T_{n,m}$ is either a trigonometric sum of order n or a polynomial of degree n . His dissertation had considered the case $m=2$ with $\rho(x) \equiv 1$, and later work by another mathematician had treated the case $m=2$ with $\rho(x)$ merely taken as nonnegative. First, with $\rho(x) \equiv 1$ [18, 19, 22, and 25], Jackson established the existence of a best $T_{n,m}$ for all n , and the convergence of $T_{n,m}$ to f as n becomes infinite, under various assumptions: $m \geq 1$, $T_{n,m}$ unique; $0 < m < 1$, $T_{n,m}$ possibly ambiguous. He then widened the scope of the results by allowing a somewhat general ρ [25 and 29]. Related research is found in several papers [31, 32, and 33]. This research systematically extended approximation theory related to $T_{n,m}$ from the case of the classical least square measure of goodness of fit ($m=2$), to the general case where merely $m > 0$, and was a significant contribution to mathematical literature.

3. *Comprehensive research reports presented to the A.M.S. by invitation.* In 1921, Jackson was the principal speaker at a symposium of the A.M.S., where he presented a synoptic account of his own main research through 1921 and related literature. This lecture was published [20]. Again, he was honored by being selected as the lecturer at the Colloquium of the A.M.S. for 1925, which involved giving a series of lectures at the summer meeting of the A.M.S. His subject was "The Theory of Approximation." He included complete presentations of essential parts of his own main research to date, associated new results, refinements of existing literature, and collateral classical material to give a rounded view of the field. His lectures were exceptional for their clarity. As usual, the A.M.S. issued a published form of the lectures as a volume in its Colloquium Series [45]. The preparation for the delivery of a series of colloquium lec-

tures, and their later publication in book form, is known to demand at least a few years of major effort from any mathematician thus honored by the A.M.S.

4. *Significant expository publications.* In the period from 1926 to 1946, Jackson devoted considerable time to expositions of fundamental content, written at the level of readers satisfying only minimum prerequisites, in order to disseminate the content more widely among mathematicians and their students, and among workers in fields of application. His principal publication of this type was his Carus Monograph, 1941, *Fourier Series and Orthogonal Polynomials* [67]. His other outstanding expositions are the three related papers [49, 52, and 53] on Fourier series. On account of these, in 1935 he was awarded the Chauvenet prize of the M.A.A. for the period 1932–34. This prize, for each three-year period, is designed to recognize the member of the M.A.A. who has published in English the most excellent exposition of valuable content. In mentioning Jackson's outstanding publications of expository type, it is appropriate here to refer again to his exceptional exposition on "Mathematical Principles in the Theory of Small Samples" [56]. In addition, his miscellaneous papers on statistics published from 1923 to 1935 in the *American Mathematical Monthly* were gems of exposition, while not pretending to be research, except to a minor extent.

5. *Detached publications in Jackson's main field from 1923 to 1946.* His research after 1925 involved several excursions, already mentioned, into the theory of statistics. He also published miscellaneous results on approximation theory apart from a coordinated major program which will be mentioned later. Thus he considered approximation to a real valued function f , whose domain is a finite real interval, by functions of particular types, not always linear families of functions [27, 38, 42, 48, and 50]. Also, he considered approximation to complex valued functions of complex variables [41, 44, 47, and 52].

6. *A relatively coordinated major program of research from 1933 to 1946,* including related theses of his doctoral candidates. First he

developed general methods with some results based on theorems of Bernstein and Markoff, for establishing an upper bound for the measure of error $h_{n,m}$ discussed in (2), and obtained corresponding convergence theorems for the sequence $\{T_{n,m}\}$ related to a function f [49, 51, 52, 55, and 57]. Of these, [51] produced useful general methods. For the remainder of his life, except for the preparation of his Carus Monograph [67], he devoted almost all of his research activity to the problem of approximating a function f of two or of three variables, where the domain of f was assumed to be of various types. In preparation for other work, he discussed [58 and 63] fundamental properties of orthogonal polynomials in two and in three variables. He obtained [61] analogs of the theorems of Bernstein and Markoff for the two-dimensional case, and then proved results for approximation, by use of polynomials $P_n(x, y)$, to a function f whose domain is a two-dimensional region R in the xy -plane. His methods here were similar to those which he had used in the one-dimensional case, with application of his generalizations of the theorems of Bernstein and Markoff.

The titles of the papers relating to orthogonal polynomials and approximation [59–61 and 63–75], and the topics of associated doctoral dissertations by his students, show that he was carrying out a systematic program. These publications attack the problems of approximation to $f(x, y)$ or to $f(x, y, z)$ or various types of point sets, mainly defined algebraically, in two or in three dimensions which can be dealt with conveniently. In a few of the publications, he synthesized general methods, and proved related theorems, apparently in an effort to indicate that the situations considered in many of his papers, and in dissertations of his students, were just particular cases of a large class of similar problems which could be proposed and solved by corresponding devices. His last publication [75], which was finished while he was a heart invalid restricted to his room at home or in a hospital, emphasized the general viewpoint mentioned in the preceding sentence. When he handed this paper to me with the request that I should arrange for its publication, he stated that it was

the last in a sequence of investigations which he had outlined in his mind, many years before, as pertinent in the theory of approximation by polynomials (and sometimes by trigonometric sums) in the two- and three-dimensional cases. Then he remarked significantly, "I am glad that I have been able to finish the job."

Jackson's enforced early retirement from active participation in national mathematical affairs at the age of fifty-two, and his death on November 6, 1946, after a long period of invalidism, were the cause of deep grief throughout the University of Minnesota, and among his former students and host of friends. His past associates recall him not only because of his scholarly attainments and his ability as a counsellor and teacher, but also because of his friendly, sympathetic personality, lively sense of humor, his high ideals, and his unswerving loyalty to his friends and his convictions. Feelings of regret at his departure are tempered by the realization that Dunham Jackson lived a very full life, working successfully in a field which he enjoyed to the hilt. In spite of his innate modesty, he could not have helped realizing that he was continually receiving the most precious of all rewards, the satisfaction of knowing that his actions and attainments in his profession and daily life were well appreciated by all of his associates, and that his affectionate feelings for them were matched by their reactions. It appears particularly fitting that I should close by quoting the final sentence in a letter which I received from a prominent mathematician in answer to a request for certain information about Jackson for this biography: "I need hardly remark in connection with your project that all those who worked for and with Jackson . . . almost worshipped him." He was that sort of man, as well as a mathematician, teacher, and educator of exceptionally high quality.

KEY TO ABBREVIATIONS

Am. J. Math.=American Journal of Mathematics

A.M.S. Coll. Pub.=American Mathematical Society Colloquium Publications

Am. Math. Month.=American Mathematical Monthly

Ann. Math.=Annals of Mathematics

Bull. A.M.S.=Bulletin of the American Mathematical Society

Carus Math. Monog.=Carus Mathematical Monographs of the Mathematical Association of America

Duke Math. J.=Duke Mathematical Journal

J. Reine Angew. Math.=Journal für die Reine und Angewandte Mathematik
Proc. Am. Acad. Arts Sci.=Proceedings of the American Academy of Arts and Sciences

Proc. Nat. Acad. Sci.=Proceedings of the National Academy of Sciences

Rend. Circ. Math. Palermo=Rendiconti del Circolo Mathematico di Palermo

Trans. A.M.S.=Transactions of the American Mathematical Society

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CHARLES KENNETH LEITH

January 20, 1875-September 13, 1956

BY D. F. HEWETT

CHARLES KENNETH LEITH was an extraordinary person. In four separate but related fields of activity, he achieved outstanding distinction; these were as a teacher (geology and related topics at the University of Wisconsin from 1902 until he was retired in 1945), as a student and worker in the science of geology (especially in the geology of the Lake Superior region, in metamorphic rocks and in ore deposits), as a professional consultant and an operator of mines, through which he made a modest fortune, and finally, as a consultant and adviser to numerous government agencies both in peace and in war. Few men working in a scientific field have achieved such distinction and recognition. The published record of his work is impressive in its scope, quantity, and quality and reveals his versatility, his extraordinary energy, and the clarity and incisiveness of his mental processes.

EARLY LIFE

Leith was born in Trempealeau, a town on the Mississippi River in western Wisconsin, January 20, 1875. Six years later, the family moved to Madison, and he grew up under the influence of the University of Wisconsin with which he was identified most of his life. He attended the public schools of Madison and, after graduating from high school in 1892, attended the local business college. He was undecided about a career but wanted to be sure that he could earn enough money to carry him through the university. This action,



C. R. Luth

seemingly of minor importance at the time, was to prove decisive in the choice of a career, for it brought him into close contact with the man who was to inspire and guide him as long as he lived, Charles R. Van Hise. Soon after Leith completed the course in the business college, he answered an advertisement in the university newspaper by Van Hise for part-time assistance in stenography and typing. Through this association Leith became private secretary to Van Hise, who had just been appointed professor of geology; Leith soon decided to pursue a career in geology.

It is worth noting that the life of Van Hise went through an interesting cycle. Like Leith he was born in Wisconsin, attended the university, and was graduated in mining in 1879. He was appointed instructor in metallurgy, a position which he held until 1883, when he was promoted to assistant professor; in 1886 he was appointed professor of metallurgy. From 1888 to 1890 he was professor of mineralogy; from 1890 to 1892, professor of Archean and applied geology; from 1892 to 1903 he was professor of geology and head of the department. In 1903 Van Hise became president of the university and held that position until his death in 1918. The published papers of Van Hise show the evolution of his interests from metallurgy to mineralogy, to the minerals found in pre-Cambrian metamorphic rocks and to ore deposits in the pre-Cambrian rocks, especially the iron ore in the Lake Superior region and, finally, to the entire range of problems of pre-Cambrian geology. In his later years he pioneered in the study of conservation not only of minerals but of all resources. What a piece of good fortune it was for Leith that at the age of seventeen he became associated with Van Hise! Actually, the evolution of Leith's own life shows several similarities to that of Van Hise.

The art of stenography was to serve Leith well through his life. Those who saw him in conferences and gatherings in Washington during the two wars will recall that he commonly made stenographic notes in his own peculiar system on scraps of paper. The conference over, he generally crumpled them and threw them away, but there

can be no doubt that merely making the notes kept his mind focused on the discussion and improved his memory of what had passed.

When Leith was a student at Wisconsin, Van Hise was deeply involved with others in the study of the iron districts of Michigan and Minnesota for the U. S. Geological Survey. In the first three monographs on iron districts, Penokee, Marquette and Crystal Falls, the only reference to Leith is in the last, by Clements and Bayley, where it is stated, "I wish to thank C. K. Leith who has been of the greatest clerical assistance." Apparently, Leith made field trips with Van Hise and others during his student days, but he did not begin independent work in the region until 1900, when he began the study of the newly found Mesabi district. He received the degree of Bachelor of Science in 1897 and that of Doctor of Philosophy in 1901.

TEACHING AT WISCONSIN

If Leith derived inspiration and knowledge from Van Hise during undergraduate days, it is quite clear, too, that Van Hise was deeply impressed by this serious young man. In 1900, when Leith was doing graduate work at Wisconsin, Van Hise, being in charge of plans and field work in the Lake Superior region for the U. S. Geological Survey, assigned him to make a monographic study of the Mesabi iron range which had only recently been discovered. Then, in 1903, when Van Hise was chosen president of the University of Wisconsin, he picked Leith to succeed him as Professor of Geology and head of the department. Leith was then twenty-eight years old.

Leith was chairman of the department until 1934 (thirty-one years), and a member until he was retired in 1945. In his position as Professor of Geology he conducted classes and held seminars in structural geology, metamorphic geology, pre-Cambrian geology, and ore deposits; also, for many years, he conducted field trips with the students. Generally there was a fall trip to the Baraboo Range to see features in general geology and a spring trip to the iron ranges to see the rocks and ore deposits in their setting. On these trips he re-

vealed the wide range of his interests and knowledge while maintaining an informality that attracted the students and endeared him to them.

As a part of his preparation for the courses in geology he maintained comprehensive files, but generally, when he appeared in the classroom, his only equipment was a slip of paper on which were written brief stenographic notes; when the lecture was concluded, he crumpled the slip of paper and dropped it in the waste basket. Some might have thought that the notes were the only record of his preparation and that much was improvised. Those on the staff knew, however, that there was a long record of preparation. Students who heard the lectures state that they were well-ordered, that he had the subject well in hand, and that he held attention and made a deep impression. Some students recall that the opening sentence of many lectures was, "Now this is the broad, general setup of this problem." Then followed, in orderly manner, the details that fortified the general conclusions. In seminars, a statement by Leith was followed by critical questioning and there was generally a terminal question, "Well, has anyone any questions to ask?" Students agree that he held undivided attention in classes by his obvious command of the subject, by his forceful manner of speaking, and by the prestige of the work he had done.

When Leith was chairman of the department, from 1903 to 1934, staff meetings were very informal; generally the group gathered for a weekly luncheon in the library. Most would agree that during the periods of chairmanship by Van Hise and Leith the department at Wisconsin was outstanding, measured both by the attainments of the members and by the quality of the graduates.

Leith did not maintain intimate social relations with the students, but once a year the graduate students were entertained at his house. He was not readily accessible to the students outside of classrooms. To consult him in his office it was necessary to get permission from his secretary, A. C. Deming, and this was not easy. It should be noted that Leith was very fortunate in the abilities and loyalty of Deming

who, in his early years, performed the usual duties of such a position but who, as the years passed, assumed responsibility for many personal matters, even fiscal, for Leith.

As his life broadened and his activities multiplied, especially following the war of 1917-18, Leith spent more and more time away from Madison; for the last ten years of his chairmanship, he received only one half of his allotted salary from the university.

In addition to instructing young men, colleges have an opportunity and perhaps some responsibility for placing these men after graduation. With the rapid growth of the American iron and steel industry from 1900 onward, there was great need for well-trained young geologists and mining engineers and, with his ties in the Lake Superior iron districts, Leith was able to aid graduates from Wisconsin in getting positions. With his own affairs also, both in the university and outside, Leith was so prolific in ideas and plans that he was in constant need of assistance from his associates and students. In his early life he was both highly industrious and meticulous in details. Later in life he developed the plans for a piece of work and delegated the details to associates and graduate students.

SCIENTIFIC WORK

For the twenty years following his graduation Leith's research work was devoted to the problems of the Lake Superior region, especially those concerned with the rocks, their correlation, deformation, metamorphism, and the ore deposits. With regard to the Lake Superior region, it may fairly be stated that, even though there were earlier reconnaissance examinations of the resources and geology of the area, systematic studies did not begin until about 1840. Since then many eminent geologists have devoted their lives to geologic studies in the region. Van Hise, who devoted most of his life to these problems, pays high tribute to his predecessors, especially R. D. Irving. Leith was introduced to these problems by Van Hise, and in many places expresses great appreciation for the association, and for the

guidance and help received from Van Hise. A review of the published papers by Van Hise shows the evolution of his interests from mineralogy to the minerals in the rocks of the Lake Superior region, to the larger problems of the rocks, their correlation, structural features, and their ore deposits over the entire Lake Superior region. Leith's own record shows a similar evolution and it seems that he took over many of the problems where Van Hise left off when he became president of Wisconsin in 1903. Thereafter, Van Hise's chief interests centered on the nationwide problems of conservation of resources and of corporate monopolies. On the other hand, from 1920 on, Leith's principal field of interest covered national resources of minerals in peace and war and the policies of the United States and all nations toward mineral supplies and trade.

In the summer of 1900, Van Hise, while in charge of the Division of Pre-Cambrian and Metamorphic Geology of the U. S. Geological Survey, assigned Leith, then twenty-five years old, to the study of the Mesabi iron district, Minnesota. The text of Monograph 43 on the Mesabi district was transmitted by Van Hise for publication in June, 1902. Earlier there had been monographic reports on the Penokee Series of Michigan by R. D. Irving and C. R. Van Hise, on the Marquette district of Michigan by Van Hise and W. S. Bayley, and on the Crystal Falls district of Michigan by J. M. Clements and H. L. Smythe. The Mesabi report by Leith was followed by one on the Vermilion district of Minnesota by Clements and one on the Menominee district of Michigan by W. S. Bayley. Monograph 52, *The Geology of the Lake Superior region* by Van Hise and Leith, published in 1911, completed this program of study of the region and its resources. In 1935 there was published Professional Paper 184 by C. K. Leith, Andrew Leith, and Richard J. Lund, which presented a revised geologic map of the Lake Superior region and the new data accumulated since 1911. This program of study and mapping of a large area with many complex problems, largely planned by Van Hise, was about the most ambitious ever undertaken by the Geological Survey.

Leith's work in the Mesabi district and field studies in the rest of the region laid the foundation for topical studies of rock cleavage (Bulletin 239, 1905), *Structural Geology* (1913, revised in 1923), *Metamorphic Geology* (with W. J. Mead, 1915). The first of these studies, Bulletin 239, assembled much data and interpreted the features that arise out of various stresses in rocks. The second and third books were modern interpretations and served as standard textbooks for many years.

Though the Mesabi Range is only a part of the Lake Superior region, Leith's work gave him a grasp on the geology of the entire region and he became the foremost authority on it. Only in the matter of the mode of origin of the Mesabi iron ores, the "soft ores," was his authority challenged. The theory arose in later years that the soft hematites may have been derived from the iron formation by hydrothermal processes instead of surface circulations, but in recent years the consensus of opinion favors Leith's interpretation.

CONSULTATION AND MINING WORK

As long as Leith held appointment on the U. S. Geological Survey he could do no consulting geologic work in the United States. From 1905 onward, however, he was much in demand as a consultant in geologic work and as a witness in litigation. He was one of the first Americans to examine the Cobalt district in Canada (1905) and in 1909 he conducted an investigation of reported iron ore on the east shore of Hudson's Bay; this episode led to the preparation of a book, *A Summer and Winter on Hudson Bay*, with the aid of his brother, A. T. Leith. In 1910 he made investigations of the iron ore deposits in central Brazil and a company, Brazilian Iron and Steel Company, was formed to exploit them. Work continued in the area for ten years but the company finally withdrew.

In 1910, with W. J. Mead, he made an examination of the extensive laterite iron ores of eastern Cuba. At this time Leith was without doubt the best informed geologist in the United States con-

cerning iron ore deposits in North and South America. It was natural, therefore, that he should be sought for guidance by some of the larger iron- and steel-producing companies of the United States. From time to time he was engaged as a witness in mining litigation but this activity was a small part of his consulting work.

Leith acquired a modest fortune from private mining ventures which began as an association with two former students, William N. Smith and Sidney H. Davis, for the operation of zinc mines in southwestern Wisconsin. Later it became the Ozark Chemical Company, operating zinc mines in the Miami area, Oklahoma, to produce zinc and sulphuric acid. Still later the Mahoning Mining Company was formed and then these were merged to become the Ozark-Mahoning Company, the principal holdings of which were fluorite properties in southern Illinois.

PUBLIC ACTIVITIES

The movement for conservation of natural resources in the United States began about 1900. Conservation of water resources in the arid regions was advocated about 1870; conservation of timber resources followed, and conservation of mineral resources began to attract wide interest about 1900. An early advocate of conservation was President Theodore Roosevelt, who initiated a national conference on conservation in 1905. Van Hise at Wisconsin was an early advocate, and in 1910 he wrote a book entitled *Conservation of Natural Resources in the United States*. From about 1900 to 1920 the United States was more than self-sufficient in most minerals and metals, and exported large quantities to Europe. The war in 1914 soon created a crisis in our shipping and made many persons in the United States aware of the dependence of Europe upon mineral supplies from the United States, and also of the dependence of the United States upon foreign sources of several mineral supplies, especially manganese, chrome, tungsten, and sulphur.

Early in the war the U. S. Shipping Board was formed to control

both exports and imports, and with the entrance of the United States into the war in 1917 there was created the Committee on Mineral Imports and Exports. E. F. Gay of the Harvard School of Business Administration was chairman of the Division of Planning and Statistics of the Shipping Board and asked his friend Leith to become chairman of the Mineral Committee. After several months of inquiry, early in 1918, the Committee devised a program of imports and exports in minerals which was placed in operation. This work completed, Leith became an adviser to the Chairman of the War Industries Board, Bernard Baruch. Through this association Leith became an adviser of the Peace Commission and spent most of 1919 in Europe. This early history is presented because it indicates the expansion of Leith's interest in national and international mineral problems that were to absorb more and more of his interest and time until his death.

Soon after the end of the war in 1918, two national engineering societies gave attention to the problems of mineral supplies in the event of war, and committees were formed to study the problems and recommend policy and action. These were the Committee on Industrial Preparedness of the American Institute of Mining Engineers, of which Leith was a member, and the Committee on Foreign and Domestic Mining Policy of the Mining and Metallurgical Society of America, of which Leith was chairman. The reports of both committees were published in 1925 in the form of a pamphlet entitled *International Control of Minerals*. In the report of the first committee a program of creating stockpiles of several of the deficient minerals was first proposed. During the next fifteen years the proposal was discussed at many meetings of groups interested in mineral supplies, and in Congress. It did not get sympathetic consideration from Congress until 1938, and the act authorizing stockpiles was not passed until 1939 (Public Law No. 117).

In 1925 and 1926 Leith was invited to lead a round-table discussion on the topic "Mineral Resources in their Political Relations," at the Sixth Conference of Institute of Politics at Williamstown,

Massachusetts. These conferences attracted eminent contemporary historians and economists from Europe as well as from the United States, and they did much to arouse general interest in mineral supply problems throughout the world. One result of Leith's activities and interests during this decade was the book, *World Minerals and World Politics*, the first to emphasize the place and influence of minerals in world affairs.

The next episode in the study of mineral problems was the formation of "The Mineral Inquiry" in 1933, sponsored by the American Institute of Mining Engineers and financed privately; Leith was chairman. During eight evening sessions in New York, eighteen speakers, largely specialists in some aspect of the mineral industries, discussed numerous problems. The papers were published in a volume entitled *The Elements of a National Mineral Policy*.

The appearance of this volume was timely, for the new administration in Washington early in 1933 created the National Planning Board, which was superseded by the National Resources Board, and a Committee on Mineral Policy held prolonged hearings and discussions in Washington. Out of this review a comprehensive report of the Planning Committee for Mineral Policy was published (1934); Leith was vice-chairman of this committee.

It is worth noting that under the Natural Resources Board there was prepared by Kenneth Leith (older son of C. K. Leith) and Donald M. Liddell, a report entitled *The Mineral Reserves of the United States and its Capacity for Production* (1936). This report was the first attempt to assemble in one volume the known mineral reserve data for thirty-one minerals and metals in the United States and to estimate the probable amount and duration of their production. Earlier such data had been assembled for petroleum, iron ore, etc. This study was inspired by Leith, as it was essential background in any efforts to form policies concerning minerals in war or in peace.

Even though there was peace among the major powers from 1919 to 1939, the rise of militant Germany during these twenty years

caused steadily growing concern among the other nations. In 1937 the Army and Navy Munitions Board appointed a Minerals Advisory Committee and requested a review of potential supplies of sixteen minerals; Leith was chairman of this committee. During 1937 comprehensive reports on sixteen deficient minerals were prepared. These reports were considered confidential and have never been published, but they were the basis of hearings in Congress, out of which came the first bill that authorized the accumulation of stockpiles of deficient minerals. This bill died in the Mining Committees of Congress in 1938 but was reintroduced in 1939 and became Public Law No. 117. Under it stockpiling of some minerals was authorized and in August, 1939, money was appropriated to purchase the minerals. War came in Europe in the late summer of 1939. Under the act, in addition to authorizing the creation of stockpiles, the Department of the Interior, through the Geological Survey and Bureau of Mines, initiated a program to search for deposits of the deficient minerals and carry on exploration.

With the abrupt collapse of France in June, 1940, new measures to control and allocate production of many materials used in the industries of the United States were needed. The President used the authority conferred on him by the Council of National Defense Act of 1916 to form the Advisory Committee to the Council. This Committee requested the National Academy of Sciences to form an Advisory Committee on Manganese, which later became the Advisory Committee on Metals and Minerals, of which Leith was chairman. During the next three years, many reports on sources of supply of metals and mineral production, substitution, and use were prepared, first for the Office of Production Management and, after the attack on Pearl Harbor in December, 1941, for the War Production Board. The scope and high quality of these reports is a tribute to the guidance of its chairman, as well as to the members of the several committees who prepared them. It is fair to say that the guidance of the chairman was most needed early in the war in the creation of procedures to handle the conflicting interests of the producers of raw

materials, the fabricators, and the consumers. Late in the war, say from 1943 to 1945, with procedures established, concern was largely directed toward maintaining maximum production and efficient distribution.

The dependence of industry on minerals, especially during a war, is indicated by the steady growth in the number of minerals that were regarded as strategic or critical. During the First World War, only four were regarded as vital and deficient in the United States: manganese, chrome, tungsten, and sulphur. As of 1939 when war in Europe began, the Army and Navy Munitions Board list included nine: seven metals and quartz and mica. Late in the Second World War, fifty-two minerals and metals were either strategic or critical.

With the termination of the war late in 1945, the war agencies were quickly dissolved and the personnel dispersed. Anticipating the problems of the peace, Leith with an intimate associate, J. W. Furness of the U. S. Bureau of Mines, and Miss Cleona Lewis of the Brookings Institution, had devoted considerable time to the preparation of a book, *World Minerals and World Peace* (1943).

This memorial is not the place to discuss the origin and evolution of the many problems that surround the discovery, exploitation, marketing, and consumption of minerals throughout a nation or the world. It is interesting, however, to trace the contributions of C. K. Leith to some of these problems. When the First World War broke, he was probably the best-informed person concerning the ferrous metal resources of North and South America, and he was familiar with these resources throughout the rest of the world. A review of his writings on these topics, beginning with the close of this war and continuing through the Second World War, reveals some of his conclusions and recommendations:

1. Important sources of minerals are distributed sporadically throughout the world, without any relation to the political boundaries of the nations; a few nations are endowed with diversity and abundance of minerals; a few have one or more minerals in great abundance and become dominant sources; most nations have modest

resources of some minerals and are deficient in many minerals; no industrial nation is completely self-sufficient in all of the minerals that it needs.

2. Some nations, possessing large resources of some minerals, sufficient to become dominant world sources, have established national controls of production and, therefore, price, solely to benefit themselves.

3. The United States, uncommonly well-endowed with resources of most minerals that are essential to an industrial nation but deficient in some essential materials, has encouraged free access to discovery and exploitation and has avoided national controls. The First World War brought out the dependency of its industries on some foreign sources and brought it into contact with the controls of some other nations.

4. The importance of (a), the so-called open-door policy with regard to minerals, or equal privileges of access by all nationals to mineral sources throughout the entire world, and (b), international control of international trade in minerals (repeatedly recommended). These policies, controlled by some sort of league of nations, would, over the years, lead to more vigorous and intelligent search for undiscovered sources of minerals throughout the world and maintain conditions of competition in exploitation that would ensure the cheapest supplies of all minerals to all nations.

5. International control of distribution of minerals, by allotting proper supplies to small and weak nations as well as to large and strong nations, would be a deterrent to war.

The rise of the spirit of nationalism throughout the world, and the creation of new nations that followed the First World War, helped to defeat the League of Nations. Also, since the end of the Second World War, more new nations have been created and the spirit of nationalism seems to dominate the world. Little that Leith and his associates recommended during the twenty years between

the wars has been accepted. It seems fair to say, however, that there is so much of idealism and practical economy in these recommendations that sooner or later the world will adopt many of them.

At least one recommendation that took form in 1921 and was emphasized in numerous reports sponsored by Leith was enacted into law in 1939. It is known as the stockpiling act, Public Law No. 117, and in recent years the policy is supported by annual appropriations. Under the present Defense Minerals Exploration Administration large stockpiles of many essential minerals and metals have been accumulated.

In a larger way, however, the activities of numerous committees and conferences of which Leith was either chairman or a member, and books of which he was the author, have finally impressed Congress, the executive departments, and especially the military units, with the vital importance of many minerals in modern industries, not only in times of peace but also, and even more so, in war, where strength is largely industrial power.

Leith's retirement from Wisconsin in 1945 coincided with the coming of peace. From then on he spent the winters in Washington and the summer months in Madison. The formal peace of 1945 did not, however, bring peace and good will among the nations, for a new type of war, the "Cold War," has divided the world and necessitated preparation for possible future conflict. From 1945 to the present, chief concern has been with the minerals uranium and thorium, the sources of nuclear energy. It was natural that Leith, even in retirement, should be called upon for consultation and advice. From 1944 until mid-1956 he was a member of the Combined Development Agency of the U. S. Atomic Energy Commission, and in 1956 he received a citation for outstanding service. He also served with the National Security Resources Board, the Research and Development Board of the Department of Defense, and the Minerals and Metals Advisory Board of the National Research Council.

THE MAN

As a casual review will show, the printed record of Leith's work is impressive in quantity, scope, and quality. This record, however, does not reveal his influence on his associates, students, and faculty at Wisconsin; on his professional associates in the mineral industries, largely in iron and steel; on groups in educational fields; on the government, and on his friends. What peculiar qualities did he possess that made him so influential in such a wide range of activities?

It seems to the writer that Leith possessed an unusual interest in life and people, a very retentive memory, and a physical and mental energy that made him work intensely over a wide range. These qualities made him interested not only in the details of a problem, but also in the broad grasp and larger picture. Until the late years of his life his horizon was steadily expanding. Few professional men have so enlarged their activities through the middle years that they continually needed and wanted help from those that surrounded them, as did Leith. In their interest in details, most professional men find it irksome if not very difficult to use assistance effectively. It seems that Leith was constantly aware of the need for help from his associates in the details of his plans and was able to make use of such help. The opening remark of many of his lectures seems very revealing: "Now, let's get the broad general picture."

Leith was a very friendly man. Even though he achieved distinction and high place in several fields, he met all kinds of people easily and his conversations and discussions were quite informal. In important conferences he was intense, but even after hours of discussion and argument he never lost his temper. As a measure of his integrity it is recalled that each of two firms involved in a legal controversy engaged him as an adviser, a rare tribute.

Leith was highly esteemed by associates, intimate and formal, and many honors came to him. To mention only a few, he was elected to high office in several professional societies, being president of the Geological Society of America (1933), and of the Society of Eco-

nomic Geologists (1925). Both of these societies awarded him their Penrose Medal (G.S.A., 1942, S.E.G., 1935). Four colleges and universities awarded honorary degrees to Leith: Kenyon College, LL.D., 1926; Lawrence College, D.Sc., 1930; Columbia University, D.Sc., 1940; Stevens Institute of Technology, D.Sc., 1943. In June, 1956, the University of Wisconsin awarded him an honorary degree, Doctor of Science. He was elected to numerous societies, several of them honorary, including the National Academy of Sciences (1920), and the American Academy of Arts and Sciences (1916).

In January, 1898, Leith married Mary E. Mayers, the daughter of a Madison merchant. To them were born two sons, Kenneth, who died in 1940, and Andrew, now associated with the E. J. Lavino Company of Philadelphia.

Charles Kenneth Leith died on September 13, 1956, at the age of eighty-one.

KEY TO ABBREVIATIONS

Ann. Am. Acad. Polit. Soc. Sci.=Annals of the American Academy of Political and Social Science

A.I.M.E.=American Institute of Mining Engineers

Bull. Am. Inst. Min. Eng.=Bulletin of the American Institute of Mining Engineers

Bull. Geol. Soc. Am.=Bulletin of the Geological Society of America

Bull. Min. Met. Soc. Am.=Bulletin of the Mining and Metallurgical Society of America

Econ. Geol.=Economic Geology

Eng. Min. Jour.=Engineering and Mining Journal

Geol. Soc. Am.=Geological Society of America

Jour. Am. Zinc Inst.=Journal of the American Zinc Institute

Jour. Canadian Min. Inst.=Journal of the Canadian Mining Institute

Jour. Geol.=Journal of Geology

Jour. Western Soc. Eng.=Journal of the Western Society of Engineers

Min. Cong. Jour.=The Mining Congress Journal

Min. Met.=Mining and Metallurgy

Ont. Bur. Mines Rept.=Ontario Bureau of Mines Report

Pan Am. Sci. Cong.=Pan American Scientific Congress

Proc. Am. Sci. Cong.=Proceedings of the American Scientific Congress

Proc. Geol. Soc. Am.=Proceedings of the Geological Society of America

Proc. Int. Geol. Cong.=Proceedings of the International Geological Congress

Proc. Int. Student Assembly Am. Univ.=Proceedings of the International Student Assembly, American University

Proc. Nat. Tax Conf.=Proceedings of the National Tax Conference

Trans. Am. Inst. Min. Eng.=Transactions of the American Institute of Mining Engineers

Trans. Am. Inst. Min. Met. Eng.=Transactions of the American Institute of Mining and Metallurgical Engineers

Trans. Can. Min. Inst.=Transactions of the Canadian Mining Institute

U.S. Geol. Surv. Ann. Rept.=United States Geological Survey Annual Report

U.S. Geol. Surv. Bull.=United States Geological Survey Bulletin

U.S. Geol. Surv. Mon.=United States Geological Survey Monographs

U.S. Geol. Surv. Min. Res. U.S.=United States Geological Survey, Mineral Resources of the United States

U.S. Geol. Surv. Prof. Pa.=United States Geological Survey Professional Papers

Univ. Wis. Bull.=University of Wisconsin Bulletin

Wis. Eng.=Wisconsin Engineer

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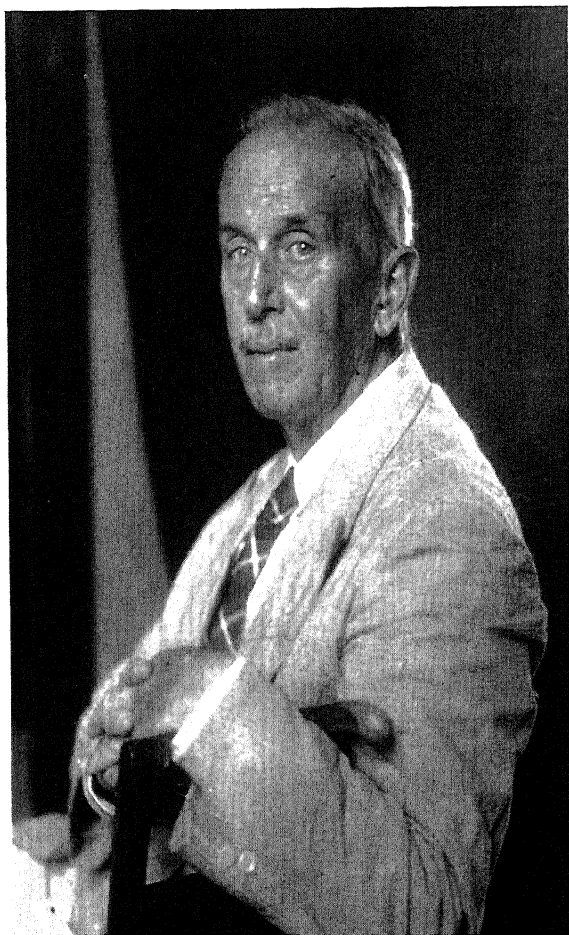
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Harold C. Longcope.

WARFIELD THEOBALD LONGCOPE

March 29, 1877-April 25, 1953

BY WILLIAM S. TILLET

WARFIELD THEOBALD LONGCOPE was born in Baltimore, Maryland, on March 29, 1877, the son of George von S. and Ruth Theobald Longcope. He was the oldest of the children in the family of three boys and one girl. In his acceptance of the Kober Medal of the Association of American Physicians, awarded to him in 1948, Dr. Longcope said: "Whatever the geneticist may say, we must owe much of what we are to our ancestors. If we can blame them for our misdeeds, so must we be grateful to them for the genes that afford us a more acceptable inheritance. Old Nathan Smith of Dartmouth and Yale, my great-great-grandfather, who treated typhoid fever in the latter part of the eighteenth century by hydrotherapy must have been richly endowed with medical genes, for there have been doctors in every generation of the family since that time." His mother's brother, Dr. Samuel Theobald, was the first Professor of Ophthalmology at The Johns Hopkins Medical School.

His father died just as Dr. Longcope was entering medical school and left him with a sense of financial responsibility for the family, a circumstance which restricted the extent of his extracurricular activities during a considerable period of his early medical career.

In his schooling in Baltimore, he first attended a private school under the direction of Dr. Deichman. He received his undergraduate collegiate education at Johns Hopkins University, graduating with an A.B. degree in the class of 1897. He straightaway entered the Hopkins Medical School, receiving an M.D. degree in 1901.

On December 2, 1915, he married Janet Dana. To them were born four children: Barbara (Mrs. Fenwick Keyser), Duncan, Mary Lee (Mrs. M. L. Johansen), and Christopher, who has followed his father's career in medicine, having graduated from the Hopkins Medical School in 1953, and is at present (1957) serving as a medical officer in the U. S. Navy.

In 1946, in his seventieth year, Dr. Longcope retired as Professor of Medicine in The Johns Hopkins Medical School and as Physician-in-Chief to The Johns Hopkins Hospital, positions which he had held for twenty-four years.

He and Mrs. Longcope then went to live in their summer home at Lee, Massachusetts, in the Berkshires. But, as will be evident from a subsequent account of his career, he was by no means idle, even though officially retired.

He died at his home in Lee on April 25, 1953, at the age of seventy-six.

When one reviews the academic activities through which Dr. Longcope developed in relation to the direction and form that academic medicine was beginning to assume in the succeeding decade after his graduation, it becomes clear that he had, unwittingly, prepared himself for the newly conceived form of full-time medicine for clinical departments that was being developed. Although not the first full-time Professor of Medicine at Johns Hopkins, after the untimely death of Dr. T. C. Janeway and the temporary acting professorships of the war and postwar years, he, with his special type of training and investigational interests, was conspicuous in being peculiarly well suited to carry forward at Hopkins the broad concept of a department of clinical medicine as being the fusion in its members of research, patient care, and teaching.

It is of interest to see how the man with his special interests and capabilities proceeded in the special academic environment to develop in it and then extend it to others for its fullest values. It is informative, therefore, to trace Dr. Longcope's career following graduation, because not only did he himself develop, but his growth

reflects what was happening historically to medical education during that same period.

Dr. Longcope entered the area of clinical medicine through the door of pathology, which, at the time of his graduation from medical school, was rapidly emerging in this country as the discipline of medical science that dominated the study and understanding of disease. William H. Welch had brought the full force of his effective influence into academic medicine through the introduction at the founding of The Johns Hopkins Medical School of the modern pathology of the day as it had been developed in Germany during the latter part of the nineteenth century.

William Osler, the first Professor of Medicine at Johns Hopkins, further advanced the importance of morphological pathology by his constant correlation of clinical observations at the bedside with the findings on the autopsy table.

It is easy to understand, therefore, how the teachings and points of view of Welch and Osler dominated the thinking of student Longcope. Graduating in 1901, he was in the fifth class following the founding of the Hopkins Medical School and consequently partook of the scholarly advantages of the Hopkins concept of medical education as being a true graduate exercise both in character and in point of view.

Accordingly, following his acquisition of the M.D. degree, Dr. Longcope's first position was Resident Pathologist at the Pennsylvania Hospital in Philadelphia. He served in this position for three years under the guidance of Dr. Simon Flexner. In 1904 he replaced Dr. Flexner as Director of the Ayer Clinical Laboratory and served in this capacity until 1911.

Although at the time it perhaps was not apparent, by hindsight it seems clear that Dr. Longcope, in selecting the type of position made available in the Ayer Clinical Laboratory for his first post-graduate activity, was creating a type of clinical activity that was to develop later into the full-time academic medical education that is essentially universal at the present time.

It is of some interest to elaborate this point. Prior to the construction of the Ayer Laboratory, the previously existing clinical laboratory of the Pennsylvania Hospital has been described as "a dark and evil smelling corner in the basement, mostly reserved for urine examinations." It is understandable that at that time no clinician had any prime interest in the activities of the laboratory, which served only as a minor accessory to the physical examination of the patient under the conditions of a well-planned bedside manner. However, when in 1898 the new Ayer Building was completed, light and airy, and equipped with new apparatus, the harbinger of a new place for the laboratory in clinical medicine had arrived, and in time workers in the laboratory would assume a dignified and respected position in promoting the best medical care for patients and in a broader understanding of the processes of disease.

At the beginning of this era, although the medical profession gave lip service to the value of laboratory activities, Dr. Longcope it has been stated, was regarded by his colleagues as sort of "wacky" in spending so much time in the laboratory when he might have been practicing medicine. Furthermore, the bacteria with which Dr. Longcope concerned himself in the laboratory were still somewhat "theoretical" in practical medicine. As a result Dr. Longcope was known in some Philadelphia medical circles as "Bugs" Longcope.

The scope of Dr. Longcope's activities in the Ayer Laboratory included pathology, biochemistry, bacteriology, and serology. And in this connection it is important to note that he performed the tests, prepared the reagents, analyzed the results, and exhibited, as attested by his extant notebooks, that he also analyzed and studied the results for their possible significance in relation to the diseases of the patients.

In addition to the laboratory activities, Dr. Longcope also visited the wards frequently and often gave valuable advice, particularly about diagnosis, based on the combined laboratory and clinical findings. In 1909 he received an additional appointment as Assistant Professor of Applied Clinical Medicine in the University of Penn-

sylvania. In this position he devoted more time to clinical activities and was able to bring to them the close correlation of bedside clinical observations with the pathological findings derived from autopsy room and laboratory.

In these efforts, therefore, Dr. Longcope represented the first Hopkins graduate to extend to another medical school the teachings and point of view in internal medicine which Osler had initiated at the founding of The Johns Hopkins Medical School.

In 1911, Dr. Longcope became Associate Professor of Medicine at the College of Physicians and Surgeons, Columbia University, New York City. In accepting this position at the age of thirty-four, his activities and interest became permanently directed toward the combined function of clinician, teacher of internal medicine, and laboratory investigator.

When he had finished his term of service at the University of Pennsylvania, it is apparent, by reflection, that following his first eight years after graduation he emerged as a uniquely trained clinical academician who set a new standard of learning in several different scientific disciplines, all merging in a broader approach to the problems of disease and in an understanding of the nature of disease both etiologically and pathologically.

It is not unreasonable to surmise that the authorities of the Columbia University College of Physicians and Surgeons sensed the development of a new era in medicine, because in 1911 they appointed Dr. Longcope as Associate Professor of Medicine and three years later, at the age of thirty-seven, he became Bard Professor of Medicine in that institution and Director of the Medical Service of the Presbyterian Hospital, New York City. To these positions he brought the multiple-faceted sources of knowledge by means of which he developed into one of the distinguished professors of medicine both in this country and abroad.

His fulfillment of his position at Columbia was interrupted by the First World War. In August, 1917, four months after the entry of this country into active combat, he assumed active duty in Washing-

ton in the Office of the Surgeon General of the Army. He also served overseas. At that time he devoted a considerable proportion of his military assignment to studies of the two great scourges of the soldiers during the 1917-1919 period, namely, influenza and hemolytic streptococcal infections, both of which occurred in epidemic proportions and with high mortality rates.

Upon his return to civilian life, he again assumed his academic duties at Columbia University. However, in 1922 he made his final change of position by becoming Professor of Medicine, Chairman of the Department, and Physician in Chief, Johns Hopkins Hospital. He now returned to his native Baltimore and the Medical School and Hospital where, as a student, his medical interests had been first developed.

For several years he was also President of the Board of Scientific Directors of the Rockefeller Institute for Medical Research.

Following retirement from Hopkins, he took up residence at Lee, Massachusetts; he continued, however, with clinical activities and teaching. He acted as consultant for a number of hospitals in the Berkshires, participating in discussions and conferences with the local practicing physicians, much to their enlightenment. In addition he made repeated visits to the nearby Albany Medical School, where he conducted clinics for the undergraduate students. He also was adviser to research groups both at that Medical School and at the New York State Department of Health Laboratories.

It is obvious from what has been written in the foregoing paragraphs that Dr. Longcope, following his first position after graduation, rose through the academic ranks to full professorial stature in two of the country's outstanding medical institutions. This recognition by the universities attests to his basic proficiency. Yet it leaves undescribed the impact of the man and his personality on the field of his endeavors, on his associates and students, and on the changes taking place in academic medicine during his period of activity.

In physical size he was somewhat smaller in height and lighter in poundage than the average. He was not brusque in his manner,

nor did he give external evidence of inner turmoils. His approach to people was friendly and gentle, cultural and intellectual. His effectiveness was not through the medium of imposing through dynamic force his ideas on his associates and pupils, but his influence was nonetheless effective.

One of his former young associates has said of him: "The other thing that impressed me about the Professor was that with very rare exceptions his office door was always quite literally open and access to him was available at almost all times. I am sure that this made his life more difficult but it did a great deal for the spirit of the Department. Finally, as you know as well as I, he and Mrs. Longcope, through extensive planning, continually opened their home to the younger generation of medical people." Said another: "The Professor exhibited a wonderful blend of noninterference with the research activities of members of his department, combined with eagerness and willingness at any time to sit down and exchange ideas and contribute sound suggestions for developing further experimentations. His fund of knowledge in all the preclinical and clinical disciplines of medicine was surprisingly extensive."

This fine academic and personal relationship between Longcope and his associates is indicated by the large number of them that later went on to responsible positions in the academic medical life of the nation.

In reviewing his earliest bibliography, the effects of contact with Welch and Osler became apparent through the interests in the field of experimental pathology and infectious diseases. Although there is an apparent diversity of subjects studied at the beginning, the circumstance is reconciled by recognizing that Dr. Longcope was himself working in both bacteriology and pathology in the Ayer Laboratories, and under these conditions he made contact, in these relatively new fields, with new findings that were exciting to him. Accordingly, "Bugs" Longcope described in early articles such findings as tuberculosis of the aorta, micrococcus zymogenes, paracolon infections, streptococcus mucosus. One of his earliest continuous

studies was that upon syphilis of the aorta which correlated in a thorough manner the pathological, etiological, clinical, and therapeutic aspects of this disease. Still another study which emerged from his environment of pathology was Hodgkin's Disease. His first publication on this subject was in 1903. Subsequently he wrote comprehensive pathologico-clinical monographs for Osler's *Modern Medicine* and *Oxford Medicine*. In addition, over many years he made intermittent efforts to establish the etiological agent of Hodgkin's Disease. He never published these negative results, but he surveyed the field of detailed bacteriology and, to some extent, virology.

In the ensuing years following his appointment to the Columbia College of Physicians and Surgeons, Longcope's research activities took a particular direction which was maintained throughout the remainder of his academic career. The scope of the studies involved immunological phenomena, perhaps best described as altered tissue responses (allergy) to foreign agents, both nonviable, such as foreign proteins or bacterial products, and viable bacteria. In 1915 the title of his Harvey Lecture was "The Susceptibility of Man to Foreign Proteins."

With his background of familiarity with both laboratory procedures and clinical manifestations of disease in patients, he was particularly well suited to explore this field.

With the advent of the therapeutic use of specific antitoxins and antibacterial sera derived from animals, and their subsequent widespread development as specific methods of treatment for severe acute bacterial infections, the foreign protein effects in man, ranging from the anaphylactic type of responses to the so-called serum sickness, became topics of increasing interest. From studies of these types of artificially induced human ailments, speculation broadened to consider the possibility that perhaps some of the "natural" diseases of man that follow acute infections might be based upon a mechanism similar to serum sickness, in which instances the source of the incitant of the disease would be foreign protein of either the bacterial bodies themselves or products elaborated by the offending organisms.

One notes in his bibliography Dr. Longcope's first interest in this subject in an article entitled "The Production of Experimental Nephritis by Repeated Protein Injections," published in 1913. Continuously after that study, he approached the problem of nephritis, particularly acute hemorrhagic nephritis, which was his major problem throughout the remainder of his career, through experimentation that involved the possibility that nephritis was an altered tissue response to the bacteria (usually hemolytic streptococci) of the acute infection that preceded the development of nephritis in much the same manner that serum sickness developed in the weeks after the therapeutic injection of immune sera.

Although the question is not yet finally settled, the point of view that acute nephritis is based upon an altered tissue response involving antigen-antibody reactions is still widely advocated.

A final intensive investigation by Longcope emerged from the Second World War. Observations in Great Britain with an antagonist for war gases, known as BAL (British Anti-Lewisite), brought out the fact that this substance promoted the excretion of metallic poisons, such as arsenic, mercury, and others. Longcope and his associates contributed to the usefulness of BAL in civilian cases of metallic poisoning.

His final clinical report, which was published six years after his retirement and based on cumulative data of both his pre- and post-retirement periods, was a comprehensive appraisal of Boeck's Sarcoid. In a reprint of the article sent to this author, his inscription read "I suppose this is my 'Swan Squawk' in clinical reporting."

BIOGRAPHICAL MEMOIRS
CURRICULUM VITAE

Born March 29, 1877, in Baltimore, Maryland. Son of George von S. and Ruth Theobald Longcope. Died April 25, 1953, in Lee, Massachusetts.

Married Janet Dana in 1915. Children: Barbara (Mrs. Fenwick Keyser), Duncan, Mary Lee (Mrs. M. L. Johansen), and Christopher Longcope, M.D.

Johns Hopkins University, A.B., 1897. Johns Hopkins University, M.D., 1901. St. John's College (Maryland), LL.D., 1934. University of Rochester School of Medicine and Dentistry, D.Sc., 1941.

Resident Pathologist, Pennsylvania Hospital, Philadelphia, 1901-04. Director, Ayer Clinical Laboratory, Pennsylvania Hospital, Philadelphia, 1904-11. Assistant Professor, Applied Medicine, University of Pennsylvania, Philadelphia, 1909-11. Associate Professor, Practice of Medicine, Columbia University, New York, 1911-14. Bard Professor, Practice of Medicine, Columbia University, New York, 1914-21. Associate Physician, Presbyterian Hospital, New York, 1911-14. Director, Medical Service, Presbyterian Hospital, New York, 1914-21. Professor of Clinical Medicine, Cornell University Medical College, February-July, 1922. Visiting Physician, 2nd Division, Bellevue Hospital, New York, February-July, 1922. Professor of Medicine, Johns Hopkins University School of Medicine, Baltimore, Maryland, 1922-46. Physician in Chief, Johns Hopkins Hospital, Baltimore, Maryland, 1922-46.

Major, Medical Officers Reserve Corps, August, 1917. Active duty in the Medical Division, Office of the Surgeon General, U. S. A., at Washington until July, 1918. February, 1918, commissioned Lieutenant-Colonel, Medical Corps, U. S. A. May, 1918, commissioned Colonel, U. S. A. Overseas with the American Expeditionary Forces until January, 1919. Upon return to America commissioned Colonel, Medical Officers Reserve Corps.

Member: Association of American Physicians (President, 1945-46); American Medical Association; American Association for the Advancement of Science; National Academy of Sciences; Society for Clinical Investigation (President, 1919); American Association of Immunologists (President, 1935); American Society for Pharmacology and Experimental Therapeutics; American Society for Experimental Pathology; American College of Physicians (Fellow); Harvey Society; Academy of Medicine, New York (Fellow); Medical and Chirurgical Faculty of Maryland; American Academy of Arts and Sciences (Fellow); Royal Society of Medicine, London (Honorary Fellow); Société des Hôpital, Paris (Honorary member); Scandinavian Congress for Internal Medicine (Honorary Fellow); Interurban Clinical Club; Century Association, New York; Hamilton Street Club, Baltimore.

KEY TO ABBREVIATIONS

- Amer. Jour. Med. Sci.=American Journal of Medical Science
Amer. Jour. Obst.=American Journal of Obstetrics and Diseases of Women and Children
Amer. Med.=American Medicine
Amer. Rev. Tuberc.=American Review of Tuberculosis
Ann. Int. Med.=Annals of Internal Medicine
Arch. Int. Med.=Archives of Internal Medicine
Boston Med. Surg. Jour.=Boston Medical and Surgical Journal
Bull. Ayer Clin. Lab.=Bulletin of the Ayer Clinical Laboratory of Pennsylvania Hospital
Bull. Hist. Med.=Bulletin of the History of Medicine
Bull. New Eng. Med. Center=Bulletin of the New England Medical Center
Centralbl. f. Bakt. Parasit.=Centralblatt für Bakteriologie, Parasitenkunde und Infektionskrankheiten
Cleveland Med. Jour.=Cleveland Medical Journal
Educ. Rev.=The Educational Review
Internat. Clin.=International Clinics
Johns Hopkins Hosp. Bull.=Johns Hopkins Hospital Bulletin
Jour. Amer. Med. Assn.=Journal of the American Medical Association
Jour. Clin. Invest.=Journal of Clinical Investigation
Jour. Exp. Med.=Journal of Experimental Medicine
Jour. Hyg.=Journal of Hygiene
Jour. Immunol.=Journal of Immunology
Jour. Med. Res.=Journal of Medical Research
Jour. Mt. Sinai Hosp.=Journal of Mt. Sinai Hospital
Jour. Pediat.=Journal of Pediatrics
Jour. Urol.=Journal of Urology
L. I. M. J.=Long Island Medical Journal
Med. Clin. North Amer.=Medical Clinics of North America
Med. Rec.=Medical Record
Minn. Med.=Minnesota Medicine
New Eng. Jour. Med.=New England Journal of Medicine
N. Y. Med. Jour.=New York Medical Journal
Occup. Med.=Occupational Medicine
Physiol. Rev.=Physiological Reviews
Proc. Inter State Post Grad. Med. Assembly North Amer.=Proceedings of the Inter State Post Graduate Medical Assembly of North America
Proc. N.Y. State Assn. Pub. Health=Proceedings of the New York State Association of Public Health
Proc. Roy. Soc. Med.=Proceedings of the Royal Society of Medicine
Proc. Soc. Adv. Clin. Res.=Proceedings of the Society for the Advancement of Clinical Research

- Proc. Soc. Exp. Biol. Med.=Proceedings of the Society for Experimental Biology and Medicine
Trans. Amer. Clin. Climat. Assn.=Transactions of the American Clinic and Climatological Association
Trans. Assn. Amer. Phys.=Transactions of the Association of American Physicians
Univ. Penna. Med. Bull.=University of Pennsylvania Medical Bulletin
Vet. Admin. Tech. Bull.=Veterans Administration Technical Bulletin
Wisc. Med. Jour.=Wisconsin Medical Journal

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PAUL DYER MERICA

March 17, 1889–October 20, 1957

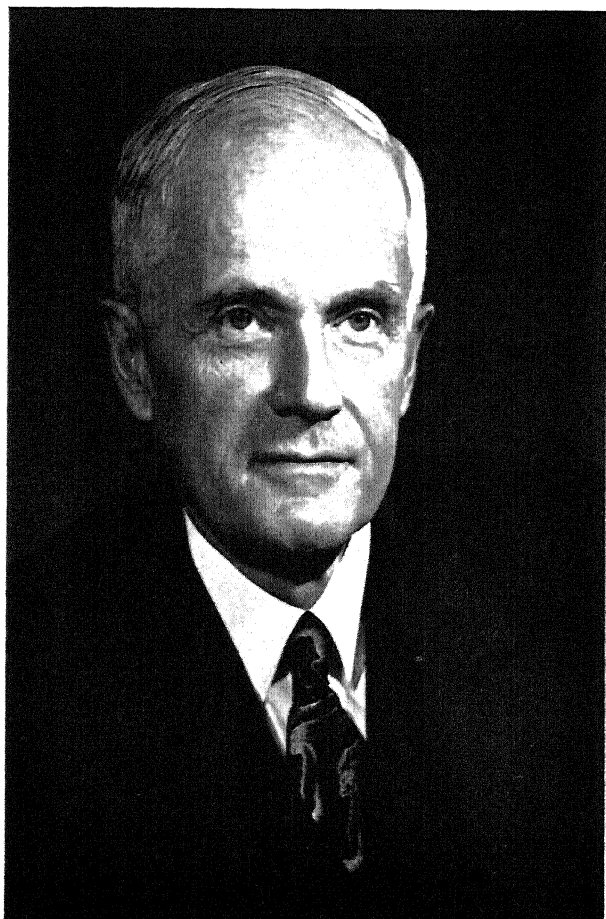
BY ZAY JEFFRIES

DR. MERICA was elected to membership in the National Academy of Sciences in 1942, as a member of the Engineering Section. He died in Tarrytown, New York, at the age of sixty-eight, on October 20, 1957, following a heart attack.

Dr. Merica was born in Warsaw, Indiana, on March 17, 1889, the son of Charles Oliver and Alice White Merica. He attended the Warsaw High School and then spent three years at De Pauw University. In 1907 he went to the University of Wisconsin, where he obtained his A. B. degree in 1908. The following school year, he was instructor in physics at Wisconsin after which he taught "Western subjects" for two years in the Chekiang Provincial College at Hangchow, China. Next he attended the University of Berlin from which he received his Ph.D. in 1914.

After his formal schooling in Berlin, Dr. Merica took a temporary post as special investigator on caustic embrittlement of steel at the University of Illinois. He then went to the National Bureau of Standards, where he remained from 1914 to 1919, holding positions as research physicist, associate physicist, physicist, and metallurgist.

In 1919 he was employed by The International Nickel Company, an association which lasted thirty-eight years. Starting at its Orford Refinery in Bayonne, New Jersey, as physical metallurgist, he soon became Superintendent of Research and subsequently Director of Research. In 1931 he was transferred to the New York office and



Gene Merina

was made Technical Assistant to the president of the parent company, The International Nickel Company of Canada, Limited, and in 1932 assistant to the president. He became a director of the parent company in 1934, and Vice-President in 1936, remaining in general charge of the company's research activities. Dr. Merica was elected Executive Vice-President in 1949 and President in 1952, retiring at his own request from the last position in 1954. He held corresponding offices in the United States subsidiary, The International Nickel Company, Inc. After Dr. Merica's retirement as President he remained on the board of directors and was consultant to Inco officers on important projects, policy, and major research and technical activities.

In 1917 he was married to Florence Young, a native of Lexington, Missouri, who died October 20, 1955.

Although Merica received his A.B. at the age of nineteen, his doctorate came at the normal age of twenty-five. Three years of teaching between the A.B. and the Ph.D. obviously resulted in an unusual degree of maturity when he was ready to begin his professional career. In fact, he could have selected either physics or chemistry instead of the field of his choice, metallurgy. He was fortunate in having the opportunity to work with some distinguished educators in the three fields—in chemistry, Professor Blanchard at De Pauw, Professor Fischer at Berlin, and Professor Parr at Illinois; in physics, Professor Mendenhall at Wisconsin, and in metallurgy, Professors Guertler and Hanemann in Berlin. His first major association at the Bureau of Standards was with Dr. Burgess, a noted physicist and metallurgist. Since metallurgical problems were both pressing and interesting during the First World War, Merica devoted most of his energy to metallurgy. The excellence of his work in metallurgy was in part responsible for his employment by the International Nickel Company.

Although Dr. Merica was at the Bureau of Standards only five years, his work there made a lasting impression on the metallurgical world. One could elaborate on his work at the Bureau, but it will

suffice here to describe briefly what may be considered not only his major contribution while at the Bureau but possibly his major contribution to science and technology.

A few years prior to 1914, Alfred Wilm in Germany had produced a new wrought aluminum alloy called Duralumin, which had some strange properties that made it singularly useful in aircraft construction. It contained about 4 percent copper and $\frac{1}{2}$ percent each of magnesium and manganese. When mechanically worked and cooled rapidly from a temperature near 500°C , its strength if tested immediately was around 40,000 pounds per square inch. If allowed to remain at room temperature, the strength increased gradually to about 60,000 pounds per square inch in about four days. The hardness and yield strength also increased markedly. Wilm did not know why his alloy behaved in this strange manner. The phenomenon was so puzzling, and so many metallurgists had sought in vain for a satisfactory explanation, that it presented a challenge to Dr. Merica and his associates.

They began work on the problem at the Bureau of Standards during the First World War. Their beautiful solution was given to the world in a paper, "Heat Treatment of Duralumin," American Institute of Mining and Metallurgical Engineers (AIME) Bulletin, June, 1919. The coauthors were Waltenberg and Scott. This work resulted in a new and fundamental metallurgical concept now known as "precipitation hardening," which has had a profound influence on both theoretical and practical metallurgy. It was said of this work that Dr. Merica fired the metallurgical shot heard 'round the world.

Since the publication of this work, dozens of precipitation hardening alloys, comprising nearly all of the common metals, have been developed. Merica was personally and deeply concerned in the development of hardenable nickel-copper alloys, an example being Inco's "K" Monel. New precipitation hardening alloys are being developed year after year. In fact, it may now be said that precipitation hardening is the most general of the hardening principles and that the hardening of carbon steel comes under a special classification.

When a publication like the above is authored by more than one person, there may be some question as to leadership in connection with the accomplishments. In the case of precipitation hardening, this matter is clear, as evidenced by letters from Mr. R. G. Waltenberg and Mr. Howard Scott, both of whom are metallurgists of note. Waltenberg wrote on January 18, 1938, "There is no doubt that any commendation for this work should be credited mainly to the direction and abilities of Dr. Merica. It gives me pleasure to think that my part in that work may contribute in some small measure to praise for Dr. Merica." On January 19, 1938, Scott wrote, "It is very gratifying to hear that Dr. Merica will receive the John Fritz Medal. . . . Dr. Merica proposed the theory of hardening due to precipitation of a compound, which is, of course, the major contribution. The text is his also, except as modified slightly to conform with the co-authors' views."

Before Dr. Merica's work, it was generally believed that the higher the degree of dispersion of a solvent metal in a solute metal the higher should be the hardness. Merica's contribution calls for maximum hardness when the second phase is precipitated in particles very small but of a critical size. If the particles are smaller than this critical size, the hardness is not maximum. The baffling nature of the problem arose partly because the "critical size" particles were submicroscopic, and very ingenious experiments and interpretations were required to arrive at the correct solution. The invention, many years later, of the electron microscope finally revealed the reality of these critically-sized particles.

The precipitation hardening potentiality is obtained in many alloys when one metal dissolves in another to a considerable extent at an elevated temperature but has substantially less solubility at a lower temperature. What Merica proposed in connection with Duralumin was that at 500°C most of the copper content of the Duralumin alloy was dissolved in the aluminum. Quenching, according to Merica, retained the copper in atomic dispersion, but this dispersion was sufficiently unstable that precipitation of a compound

CuAl_2 took place slowly at room temperature. It seems fortuitous that Duralumin was capable of undergoing this internal change at room temperature and yet was not capable of growing the CuAl_2 particles to larger than critical size. As work progressed, it was possible to generalize with reference to precipitation hardening principles. Sometimes the secondary precipitation would not take place at room temperature but it would take place at some specified higher temperature. It therefore became possible with many alloys to develop heat treatment procedures which would produce either maximum hardness or any degree of hardness desired between maximum and that in the as quenched condition. Nearly all of these results flow logically from the original concepts of Merica.

Dr. Merica entered the nickel business at a most critical time. Up until 1918 the prosperity of the world's nickel industry, in which International Nickel was a dominant factor, had depended mainly on armaments. When nickel demand practically disappeared in 1919-21, it was realized that great efforts had to be directed to the discovery of new peacetime uses for nickel. Dr. Merica joined in this great task with his characteristic vigor and determination.

Dr. Merica continued to make contributions in the field of metallurgy after he became associated with International Nickel. He improved the nickel melting processes and developed new alloys and combinations, such as Inconel and "K" Monel. He did notable work in connection with extensive utilization not only of nickel but also of other alloying elements in cast iron. Merica was not the first to add nickel to cast iron, but he showed how it could be used advantageously in the complex technology of cast iron. Not only did he contribute to the low nickel cast irons, but with his associates he developed a cast iron alloy called Ni-Hard with an intermediate amount of nickel which was highly resistant to abrasion, and another cast iron alloy called Ni-Resist with a large amount of nickel, which was highly resistant to corrosion. These alloys and their several modifications are still used commercially. Merica also had a hand in many other metallurgical developments in connection with

nickel and gave sympathetic support to the development of ductile or nodular cast iron, a variety of cast iron made ductile at room temperature by the addition of a small amount of magnesium.

Merica always considered himself as basically a physical chemist, deflected by chance into metallurgy. But his days as a working scientist were cut short by his executive talents, which soon led to his being drafted into high management areas. From 1922 onwards his scientific position was perforce that of a leader and inspirer of others. For more than a decade he was a frequent visitor at the Inco Bayonne Research Laboratories, showing a contagious interest in all that was going on and quick to perceive the crucial issue in an obscurely unfolding research. He was a strong believer in the necessity for clear objectives, feeling that even an unobtainable goal lent force, direction, and productivity to research. His combination of scientific competence with industrial acumen led to developments which have retained their commercial utility for many years. His metallurgical inventions are represented by twenty United States patents, many of which have foreign counterparts. It is characteristic that all but two of these are jointly with others.

In his position as one of the leaders of Inco, Merica was also interested in the large-scale and complex operations of winning metals from their ores. During the last decade of Merica's life International Nickel's mines in the Sudbury District of Ontario were greatly expanded, entirely new metallurgical processes were introduced, and the recovery of the valuable metal content of the ore was improved. Nickel, copper, platinum, palladium, rhodium, ruthenium, iridium, gold, silver, cobalt, selenium, and tellurium are recovered in metallic form. Important tonnages of sulphur are also recovered in the form of sulphuric acid and liquid sulphur dioxide. Finally, iron is going to market by the hundreds of thousands of tons per year as a high grade iron ore. Merica was a key figure in this program.

Dr. Merica was also active on the Inco team in appraising the ores of the Mystery-Moak Lakes area in Northern Manitoba and in planning to bring these important properties into production.

Dr. Merica in later life spent a large part of his energy in managing, planning, and generally doing those tasks which only the captains of industry can do. However, he never ceased to be a scientist and a technologist. His professional standing can be judged merely by mentioning some of the honors he has received and some of his associations with professional societies. In 1929 he was awarded the James Douglas Gold Medal of the American Institute of Mining and Metallurgical Engineers; in 1938 he was awarded the John Fritz Gold Medal by the four founder engineering societies; in 1941 he received the platinum medal of the British Institute of Metals; in 1942 he was awarded the Franklin medal by the Franklin Institute, and in 1951 he received the gold medal of the American Society for Metals.

Among other associations he was a fellow of the American Association for the Advancement of Science, and a member of the American Iron and Steel Institute, American Chemical Society, American Society for Testing Materials, American Physical Society, Institute of Metals and the Iron and Steel Institute (both of Great Britain), and Canadian Institute of Mining and Metallurgy. He was quite active in some of the societies, having been a member of the executive committee of the American Society for Testing Materials, a member of the board of directors of the American Society for Steel Treating, Vice-President and a member of the board of directors of The American Institute of Mining, Metallurgical, and Petroleum Engineers, and Vice-President of the American Electrochemical Society.

Dr. Merica received honorary doctor of science degrees from De Pauw University in 1934, from Lehigh University in 1938, and from Stevens Institute of Technology in 1942. He was made an honorary member of the American Institute of Mining, Metallurgical, and Petroleum Engineers in 1942. In 1955, he was made an honorary member of the American Society for Metals, becoming the twenty-second person to be so honored, joining a roster of distinguished scientists and engineers, both past and present.

Dr. Merica contributed generously to the technical literature, hav-

ing presented upwards of fifty papers, and in addition he gave many lectures. It is interesting to note that he gave the invitational lecture on nonferrous metals at the Institute of Metals Division of the AIME in 1932 and the Howe Memorial lecture on cast iron, sponsored by the Iron and Steel Division of the same Institute in 1937.

His ability to lecture authoritatively in both nonferrous metallurgy and ferrous metallurgy was not surprising to his acquaintances. Dr. Merica's versatility was recognized when he received the John Fritz Medal, and was presented as "scientist, engineer, inventor, teacher, executive, philosopher and benefactor of mankind." To this should have been added "one of the world's foremost metallurgists." He had a keen appreciation of music, and engaged in athletics normal to his time. In baseball he was a catcher, and one of his associates at the Bureau of Standards made the following observation about him: "It seems that I can't keep up with Merica in anything, not even tennis, at which I consider myself quite good."

Precociousness, as evidenced by the award of the A.B. degree at the age of nineteen, characterized Merica's whole life. His advice was sought by many people, because he had exceptionally good judgment on many matters. He worked very closely in the International Nickel Company with the late President and Chairman, Robert C. Stanley.

Stanley wrote of him, "Dr. Merica combines quick perception, fast thinking and accuracy in an unusual degree. He combines great scientific ability with a keen commercial sense. He can draw a contract as ably as a lawyer. He is a philosopher and a practical economist. He thinks in world terms. His studies and teachings in foreign countries, coupled with his later experience in connection with international matters, have been important factors in the development of this cosmopolitan thinking."

In formally reporting Dr. Merica's death to his fellow members on the Board of Directors of International Nickel, Chairman John F. Thompson and President Henry S. Wingate referred to his great achievements. They added that "apart from his achievements, Dr.

Merica will probably best be remembered because of his fine qualities and the depth and integrity of his character. These endeared him not only to his associates in the company but throughout the nickel industry. We have suffered a great loss." Indeed Dr. Merica had a host of friends and admirers, who are keenly conscious of the loss caused by the passing of a great man.

KEY TO ABBREVIATIONS

- Bur. Standards Circ.=U.S. National Bureau of Standards, Circular
Bur. Standards Sci. Paper=U.S. National Bureau of Standards, Scientific Paper
Bur. Standards Technol. Paper=U.S. National Bureau of Standards, Technologic Paper
Can. Mining J.=Canadian Mining Journal
Chem. Met. Eng.=Chemical and Metallurgical Engineering
Ind. Eng. Chem.=Industrial and Engineering Chemistry
Intern. Z. Metallographie=Internationale Zeitschrift für Metallographie
J. Am. Inst. Metals=Journal of the American Institute of Metals
J. Am. Welding Soc.=Journal of the American Welding Society
J. Franklin Inst.=Journal of the Franklin Institute
Metal Ind. (N.Y.)=Metal Industry (New York)
Met. Chem. Eng.=Metallurgical and Chemical Engineering
Mining Met.=Mining and Metallurgy
Proc. ASTM=Proceedings of the American Society for Testing Materials
Trans. Am. Inst. Chem. Engrs.=Transactions of the American Institute of Chemical Engineers
Trans. Am. Inst. Metals=Transactions of the American Institute of Metals
Trans. AIME=Transactions of the American Institute of Mining and Metallurgical Engineers
Trans. ASST=Transactions of the American Society for Steel Treating
Trans. Can. Inst. Min. Met.=Transactions of the Canadian Institute of Mining and Metallurgy

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R. A. Millikan

ROBERT ANDREWS MILLIKAN

March 22, 1868–December 19, 1953

BY L. A. DU BRIDGE AND PAUL S. EPSTEIN

ROBERT ANDREWS MILLIKAN was without question one of America's greatest scientists. He was, at the height of his career, not only the nation's most renowned physicist but also a conspicuous educational leader and public citizen. To write his biography in a brief form is certain to do an injustice to one or more aspects of his career. But, fortunately for the authors of this memoir and for the world in general, Robert Millikan wrote his *Autobiography*, published in 1950 by Prentice-Hall, New York. In that important and fascinating book of 300 pages, he himself sets forth the story of his life—his schooling, his experiences as a teacher and research physicist, his building of Caltech, his work in two world wars, and last but not least, a generous introduction to his own philosophy of life and of science.

To the *Autobiography* the interested reader must be referred for a full account of these matters. It will be our intention here to devote primary attention to examining and evaluating his contributions to physics and to describing his leadership in building the modern California Institute of Technology.

BIOGRAPHICAL OUTLINE

Robert Millikan was born on March 22, 1868, in Morrison, Illinois, the son of a preacher, and grandson of one of the early settlers (1834) of the Mississippi River country of western Illinois. At the age of

five his family moved to McGregor, Iowa, and two years later to Maquoketa, Iowa, where Robert went through high school and lived the life of an Iowa country boy, not unlike Mark Twain's Tom Sawyer.

He entered Oberlin College in 1886, and took trigonometry, analytic geometry, and Greek—"the subjects which interested me most"¹—and a twelve-weeks' course in physics—"A complete loss." But, to his astonishment, he was asked to *teach* the physics course during his junior year. He prepared himself by working all the problems in the book that summer—and then making the students work them during the year. He became then, and remained all his life, a vigorous exponent of the problem method of teaching. The lecture method, he insisted, is "a stupid anachronism—a holdover from the pre-printing-press days."

He received his bachelor's degree from Oberlin in 1891 and a master's degree in 1893, and then went to study at Columbia as the sole graduate student in physics there. Michael Pupin—"the only teacher there with analytical training"—had a great influence on Millikan, in spite of the fact that Pupin—to Robert's horror—took no stock in the atomic theory of matter.

Following the receipt of his doctor's degree in 1895 Millikan decided on Pupin's insistence—"and since a satisfactory job did not appear"—to study in Germany. It was a fortunate time to be studying physics in Europe—Roentgen's discovery of X rays, for example, came in November of that year, and Becquerel's discovery of radioactivity only a few months later. Millikan frequently remarked on how fortunate he was to be born in 1868, just in time to study in Germany in the critical year 1895-96. (Others born in that year made less use of their good fortune!)

In 1896 Professor A. A. Michelson, with whom Millikan had studied at the University of Chicago during the summer of 1894,

¹ Quotations here and on the following pages, if not otherwise identified, are from the *Autobiography*.

offered him an assistantship in the Chicago department and Robert hastily terminated his sojourn in Europe to accept.

For the next twelve years at Chicago Millikan worked on a twelve-hour schedule—twelve hours *a day*—six hours teaching and writing textbooks and six hours in research. His major interest, born of necessity, was in teaching; and the text books which he and his colleagues then wrote were the foundation of most high school and college teaching of physics in the United States for the next quarter century. A descendant of one of them—by Millikan, Roller, and Watson—is in use to this day. Under Millikan's influence the physics teaching in this country "grew up"—and the spectacular development of American physics in the second quarter of the twentieth century was a direct consequence, though it was also assisted by Millikan in many other ways to be discussed later.

In 1902 Robert married Greta Blanchard and they spent their honeymoon in Europe. It was his third visit, since he had been sent by the University to the World's Fair in Paris in 1900 to set up an exhibit of some of Michelson's instruments. During all his early and later visits to Europe, Millikan made excellent use of his time and made friends of most of the leading physicists of that day. In fact, few American scientists have ever had such a host of personal and professional friends in all parts of the world. He remembered everyone he met, and corresponded extensively with many. His warm personality and wide range of interests made his friendship something to be highly valued by anyone.

After ten years at Chicago, Millikan decided to "get busy on some more serious research." He was still only an assistant professor at a salary of less than \$2,000, not counting a growing income from his books. He was not then satisfied with his achievements as a physicist, but his first "serious" experiment was a spectacular success indeed. He called it "My Oil Drop Venture (e)." In the next few years he developed the famous technique of the falling charged oil drop and used it to prove the atomicity of electric charge and also to

measure accurately the value of the universal unit of charge, the electron.

No sooner had he finished most of this long series of experiments than he began another—technically more difficult and equally significant—which established the atomic or quantum nature of light by verifying Einstein's 1905 quantum theory of the photoelectric effect. For these two magnificent experiments—to which we shall return later—Millikan was awarded the Nobel Prize in Physics in 1923.

These and other research activities, plus his normal duties as a faculty member at Chicago, his growing participation in the affairs of the American Physical Society (he became president in 1916), and other extracurricular activities kept him busy until the United States entered the First World War.

In April, 1916, the National Academy of Sciences took steps to organize the National Research Council to assist the Government in mobilizing the nation's scientific resources for preparedness. Millikan was a member of the organizing committee, along with Edwin G. Conklin, Simon Flexner, Arthur A. Noyes, and George E. Hale (chairman). Later Hale, as permanent chairman of the Council, asked Millikan to serve as Executive Officer and Director of Research. He moved to Washington in February, 1917, and spent the remainder of the war there directing the mobilization of civilian scientists and their collaboration with the military agencies. Millikan's account of those years in his *Autobiography* constitutes a historic document.

The mobilization of civilian scientists—as scientists—for war was a new thing in 1917, and the difficulties were great. However, the new ideas developed in the New London laboratories on submarine detection were so important and affected such a critically serious problem that military respect for the scientist was greatly enhanced. The advances in chemical warfare were even more significant. Had there been a mechanism for continuing the scientific work on anti-submarine warfare effectively after 1918, the submarine menace in

1942 might not again, as in 1917, have been the instrument of bringing near disaster to the Allies.

An interesting sidelight on Millikan's experience in the First World War was the fact that he—the leading civilian scientist—was urged, and consented, to accept a commission in the Army Signal Corps—as a major!

After the armistice Millikan remained in Washington most of his time until October, 1919, assisting in the permanent establishment of the National Research Council and the planning of the new building to house the Council and the Academy following the gift in June, 1918, by the Carnegie Corporation of \$5,000,000 for the purpose. Millikan personally raised a substantial share of a fund of \$160,000 required for the purchase of the site for the building, this being a condition of the Carnegie gift.

In 1918, George E. Vincent, president of The Rockefeller Foundation, requested advice on the strengthening of American science. Millikan proposed the establishment of what were to become the National Research Council Fellowships. Millikan said later that this program was "the most effective agency in the scientific development of American life and civilization that has appeared . . . in my lifetime." There are few who know the story of the rise of American science from almost nothing to full maturity in the twenty years following 1918 who could disagree. Millikan's objective was the *decentralization* of science and was contrary to a strongly supported proposal for a central national institute of science. He felt strongly that many universities throughout the country must become centers of research and that the National Research Council Fellows would be the "seed corn" of such a development. By 1940 a large share of the leading physicists and chemists of the country were former NRC Fellows and they were to be found in dozens of flourishing research centers from coast to coast. Millikan's foresight was fully justified.

In October, 1919, Millikan returned to Chicago fully expecting to carry on there with the many research programs he had undertaken and which he resumed after the wartime interruption. But the

persuasiveness of two old friends with whom he had been in intimate association during the war—George E. Hale and A. A. Noyes—plus, as he frankly admitted, a handsome offer as to salary and research funds—brought him to the new and almost unknown Throop College of Technology in Pasadena, California. He insisted that the name be changed to the California Institute of Technology, and he arrived to take up his duties in late 1921.

He also frankly stated that his main objective in going to Pasadena was “to build an outstanding department of physics.” As far as Hale and the other trustees were concerned, however, Millikan was “president”—even though he insisted that the administration be assigned to an “executive council” of which he was chairman.

At any rate, the next twenty years of his life—until the onset of the Second World War—were years of almost unbelievably intense activity. In 1921 Millikan was already fifty-three years old. At sixty-three he was at the peak of his efforts, and he did not retire until 1945, aged seventy-seven. During those years he pursued an energetic research program which made the Norman Bridge Laboratory one of the most famous physics centers in the world, and at the same time he built the California Institute from almost nothing to a position of financial stability and world preeminence. When he retired, his efforts had built an institution whose worth was \$40,000,000 and his influence was still being exhibited through bequests and gifts that were still coming in in 1956.

But enormous though these achievements were, Millikan's efforts were by no means confined to Pasadena. He never lost interest in the National Academy of Sciences and the National Research Council, and he maintained active responsibilities until 1940. He was a public citizen in a wide variety of ways, ranging all the way from active participation in two “social-literary” clubs (the Twilight and the Sunset Clubs) of Southern California to the Committee on Intellectual Cooperation of the League of Nations. He traveled all over the earth, both on lecture tours and on cosmic-ray expeditions. He was elected to most of the scientific academies of the world and ac-

cumulated more honors and awards than any American scientist of his time.

During the Second World War Caltech was immersed in an enormous war research program—expending over 80 million dollars. Though he left the active direction to others, Millikan's administrative tasks were obviously very heavy, and he followed all the projects with meticulous interest.

After his retirement in 1945 he came daily to his office in the Norman Bridge Laboratory in Pasadena, read omnivorously, kept in close touch with research in many fields, and as late as 1947 led a cosmic-ray expedition, releasing high altitude balloons at a series of remote locations from Texas to Canada.

In early 1953, at the age of eighty-five, he began to fail physically, and he passed away on December 19, 1953, only a few weeks after the death of his wife Greta.

American science and education had changed markedly in the first half of the twentieth century, and Robert Millikan took a leading part in bringing the change to pass.

MILLIKAN AS AN EDUCATOR

The supposed impossibility of being both a good research man and a good teacher was given a magnificent denial in the life of Robert Millikan. The first years of his career at Chicago were given over largely to teaching, and throughout his life his students were always close to his heart. He never forgot that the main purpose of a university was to educate young men and women. True, he also loved research for its own sake and for the new knowledge it revealed. But he loved it especially as an instrument of instruction. A major educational asset at Caltech, he insisted, was to be the atmosphere of research which was being built. He loved to take time out, day or night, for long, often rambling, but always enthralling discussions with his students. He was famous as a lecturer and often attracted thousands to the popular lectures on cosmic rays or atomic physics which he gave all over the country.

Every director of research who presides for a number of years over a large laboratory has the right to expect among his personal pupils one or two men of exceptional merit. But in Millikan's case, we find as his close associates, growing to manhood and fame in intimate collaboration with him, far more eminent physicists than can be accounted for by the law of averages. On the strength of this record, Robert Andrews Millikan must be classed as one of the most successful teachers in the history of science.

Physics had a great unity which was always dear to him. His experiments were related to everyone else's. He enjoyed every physics journal he read, for he saw in every paper a relation to work going on in his laboratory. And he conveyed this enthusiasm, these hidden relationships, to his students and colleagues.

His great contribution to American education was the building of the California Institute of Technology. There are many universities which have struggled for a hundred years to attain the excellence and prestige—and the funds—which Caltech achieved in less than twenty years. It is true that in the thirty years before 1921 Throop College had gone through its birth pangs, its growing pains, its days of penury. During that time it had acquired the respect of the community, a few able and devoted faculty members (including A. A. Noyes, chemist, and Royal Sorensen, electrical engineer) and a Board of Trustees containing some public-spirited citizens of outstanding ability—among them George Ellery Hale, the astronomer, and Henry M. Robinson, a wealthy Los Angeles business man. Southern California was an area of great promise—the time was ripe for a new educational experiment. Millikan was the man to guide it.

He had grown impatient with complex and cumbersome academic administration; he wanted to abolish departments and other artificial barriers to cooperation. He did.

He was imbued with the conviction that a great upsurge of science and technology in America was long overdue—and that some institutions should devote themselves single-mindedly to the task of edu-

cating the men who would lead that revolution. He dedicated himself to the task of creating such a place.

He was convinced that real research centers were not being built in American universities because research was still regarded as a junior partner to teaching—an activity to be carried on in a basement room on evenings and Sundays and without expense to the university. He was determined to found an institution where teaching and research went hand in hand, where a major assignment of resources to research would be achieved, where research would provide the creative atmosphere for stimulating teaching, and where young students would keep the freshness of the research spirit alive. These and other ideals became embodied in the new Caltech.

But, most of all, Millikan attracted to Caltech great men: R. C. Tolman (physics and chemistry), Paul S. Epstein (physics), Theodore von Karman (aerodynamics), Thomas Hunt Morgan (biology), Harry Bateman (mathematics), John Buwalda (geology), Chester Stock (paleontology), Beno Gutenberg (geophysics), Linus Pauling (chemistry), to mention but a few who were in residence there by 1930. In 1958, twenty-eight members of a total science faculty of 200 were members also of the National Academy of Sciences. Three were Nobel prize winners. In the year 1925 three future Nobel prize winners were students at Caltech, Carl D. Anderson, E. M. McMillan, and Linus Pauling. William Shockley, a fourth, arrived the following year.

The impact of Caltech upon the Southern California community was enormous. Millikan saw the need for electric power and began an important program in high-voltage engineering research—leading to the economical transmission of power from the Hoover Dam on the Colorado River to Los Angeles. He foresaw the growth of the aviation industry—and built a large wind tunnel, and initiated under Theodore von Karman a program of study of the *science* of aerodynamics. He took “earthquake” out of the category of forbidden words in California and established a first-class laboratory of seismology.

As Millikan bluntly stated it: "The California Institute of Technology has kept itself alive and has grown in plant and resources by the very simple technique of continuously seeking and finding ways in which it could serve in the first instance this Southern California community *with such efficiency as to convince its supporters* [italics his], whether they be philanthropic individuals, or business men, or the general public, that it is one of Southern California's greatest assets."

To serve the community through research projects and by providing educated leaders for science and industry—these remained the simple and unchanging goals. The Institute was kept small; no new projects were undertaken without adequate financing; temptations to multiply size and numbers, new schools, new divisions, new departments, were systematically avoided. Six academic divisions only were created by 1928, and six only exist today. In 1920 the freshman class was limited to 160; only in 1948 (after Millikan's retirement) was the limit raised to 180, its present level.

An insistence on a high content of basic science in the engineering curricula and a high content of basic studies in the humanities and social sciences was also a basic tenet of the Institute policy. These were not widely accepted principles in engineering schools in 1921. In 1955 they were laid down as essential qualities for engineering education by a Committee of the American Society for Engineering Education—and accepted and published by the Society. They were thirty-five years old at Caltech.

It is obviously difficult to describe Millikan's contributions as an educator without launching into a panegyric about Caltech. But few institutions in the nation are so completely the shadow of one man and, at the same time, stand out so conspicuously for their achievements. For thirty-five years Dr. Millikan was Mr. Caltech, and Caltech will always be his monument.

MILLIKAN AS A PHYSICIST²

The general characteristics of Millikan's approach to experimental research are worth noting. He began with a thorough study of the work of his predecessors, analyzing their methods with a view of discovering the weak points that could be improved upon. This enabled him to start work with an experimental setup eliminating some of the previous sources of error. Since the problems treated by Millikan were among the most difficult, an easy success in a single paper could not be expected. But even the first paper usually represented an advance over the preceding work; moreover, it gave him experience and a better understanding of the functioning of his instruments, thus enabling him to devise further improvements in his apparatus, to undertake with it a second piece of research, and to report the further progress in a second paper. He would always strive for a complete understanding of all the secondary processes taking place in his setup, often trying separate experiments to elucidate some obscure details. In this way, the very sources of error became subjects of research, leading to instructive results, and sometimes to significant discoveries. Thus, by slow degrees Millikan advanced to a complete mastery of every aspect of his problem and brought the investigation to a close, in the sense that he obtained final results which could not be improved upon with the experimental resources of the epoch.

All this required much time and hard work. Every subject of research developed into a whole program, often branching out into new subjects. Fortunately, Millikan did not have to accomplish everything singlehanded, but could delegate part of the work to his pupils. He always possessed, in a high degree, the ability for teamwork which springs from a friendly and sociable temperament. He delighted in paving the way for deserving younger men, who took their first scientific steps under his guidance, and he had much to

² This section is adapted from the article by Paul S. Epstein, "Robert Andrews Millikan as Physicist and Teacher," *Reviews of Modern Physics*, 20 (No. 1): 10-25.

offer them as the possessor of the vastest research experience and of the soundest experimental technique.

Determination of the Electronic Charge e . The investigation of the electronic charge was started by Millikan in 1907 jointly with his student L. Begeman, with a view of improving the method of H. A. Wilson, which seemed to the authors to be the most promising. This method consisted in ionizing the air in a fog chamber and condensing on the ions a cloud by means of a sudden expansion. First, the rate of fall of the cloud under gravity alone was observed, then the rate of fall of a similar cloud when a vertical electric field was superposed upon gravity. Stokes' law of resistance made it possible to obtain the mass of the droplets constituting the cloud from the velocity of their descent under gravity. The additional knowledge of the velocity in the electric field gave information about the ratio of the electric to the gravitational forces and, ultimately, about the ionic charge.

Millikan saw in the Wilson experiments certain sources of error which he set out to correct, and he at once obtained more consistent results. His first value for the charge e was 4.03×10^{-10} e.s.u., compared to Wilson's value of 3.1×10^{-10} , a considerable improvement. But other sources of error at once appeared, and Millikan set out systematically to track them down. The temperature estimates were incorrect, and this led to the use of an erroneous value for the viscosity of air. The different parts of the cloud of droplets did not all fall with the same speed, for there were different size droplets and different charges. This led him to the very great step of observing individual drops. When he then substituted oil for water to reduce the rate at which the droplets evaporated, he laid the basis for a thoroughly comprehensive and highly precise experiment—one which he worked on patiently and brilliantly for ten years.

In the final form of the experiment a cloud of oil drops is produced by an atomizer, and it turns out that a large number of them carry electric charges. The drops are allowed to fall under gravity through a hole in a plate which constitutes the upper member of an

accurately machined pair of plates between which an accurately measured electric field can be applied. A single droplet is selected for observation through a low-power microscope and its rate of fall under gravity is observed. If the viscosity of the air is known, the size and, hence, the weight of the drop is determined. The electric field is now applied in such a direction as to counteract gravity and a new rate of fall or rise is determined. If, for example, the electric field is adjusted to hold the droplet at rest, the upward force must equal the weight, and the charge is at once determined.

Occasionally the charge on a drop changes—and the frequency of change can be increased by irradiation with gamma rays. The change in charge causes a change in speed and the ratio of the old and new charges can be accurately determined. Millikan was thus able to establish beyond doubt that all charges and also the *changes* in charge were whole multiples of a least value, and the atomicity of electricity was confirmed. The attainment of a highly accurate value for this fundamental charge was, however, not so easy.

Already the first investigation with the oil-drop method showed that Stokes' formula was not accurate enough for determining the particle radii. However, the more accurate Stokes-Cunningham formula, which takes into account the dependence of the rate of fall on the mean free path, led to very consistent results and gave for the electron charge the slightly too high value of $e = 4.891 \times 10^{-19}$ e.s.u. The two final papers of the series were published in 1913 and 1917. They represent two complete and independent determinations of the electronic charge; although the method of the second investigation was the same, it was carried out with a new apparatus, and all the auxiliary constants were re-evaluated. The results were identical, namely, $(4.774 \times 0.009) \times 10^{-10}$, in the first, and $(4.774 \times 0.004) \times 10^{-10}$, in the second—a figure which essentially remained the standard value for over twenty years, though the new more accurate data for the velocity of light and the value of the absolute ohm brought it down to 4.770×10^{-10} .

These papers definitely settled the question of the uniqueness of

the electronic charge which was until then open. Even today they are definitive for the oil-drop method in the close consistency and small spread of the individual determinations. Although the investigation has been repeated by several other authors, the accuracy of Millikan's relative results has never been equaled. With respect to the absolute result, a small uncertainty lay in Harrington's value of the coefficient of viscosity of air accepted by him. In the nineteen thirties the evidence of indirect determination of the electronic charge began to accumulate and to point to the conclusion that Harrington's value was slightly too low. Hence, redeterminations were undertaken in the Norman Bridge Laboratory and elsewhere (see below) which led to results clustering about $\tau_{23}=1830.0 \times 10^7$. With this correction Millikan's determinations of 1913 and 1917 would give $e=4.799 \times 10^{-10}$ e.s.u., a value of the electronic charge which must be considered the most accurate directly obtainable by the oil-drop method. Millikan estimated its accuracy as $\frac{1}{3}$ of 1 percent, owing, primarily, to the uncertainties in the determination of the viscosity of air.

Viscosity of Air and Stokes' Law. Millikan's interest in the viscosity of air grew out of the fact that an accurate knowledge of it was needed for the evaluation of his experiments on the electronic charge. The then available data were concerned not so much with the absolute value needed by Millikan as with its relative changes in dependence on temperature and pressure. Hence, he caused several determinations to be carried out under his supervision in his Chicago laboratory. The Poiseuille method of flow through capillary tubes was used by I. M. Rapp and E. Markwell, and the method of rotating cylinders by L. Gilchrist and E. L. Harrington. It was found that the second method was capable of a higher accuracy. Especially, as perfected by Millikan and Harrington in 1916, the rotating cylinder apparatus was superior to any that had been used before that time. (Their result was given above.)

In 1930 Millikan returned to the work with rotating cylinders for the purpose of determining the viscosity of organic vapors. Jointly

with R. K. Day, he developed in the Norman Bridge Laboratory a modified apparatus which differed from Harrington's in that it could be used at high temperatures. This apparatus was used by Day to measure the viscosities of several substances in their dependence on pressure. R. K. Day worked with normal and isopentane and W. M. Bleakney with 2-pentene, trimethylethylene, and carbon tetrachloride. In all these cases the pressure coefficient was negative.

The circumstances which led to the reawakening of the interest in the absolute viscosity of air were mentioned above. After the importance of its redetermination was pointed out by K. Shiba in 1932, the work was undertaken independently by several investigators. As far as the rotating-cylinder method was concerned, the improvements were not primarily due to instrumental changes, because Houston worked with the identical apparatus built by Millikan and Day, while Kellström and Bearden used instruments of a very similar construction. The advances lay rather in a more thorough discussion of the sources of error and in more elaborate corrections for them. The results for η_{23} were as follows: Kellström $(1834.9 \pm 2.7) \times 10^{-7}$, Houston $(1829.2 \pm 4.5) \times 10^{-7}$, Rigden $(1830.34 \pm 0.6g) \times 10^{-7}$, Bearden $(1834.02 \pm 0.06) \times 10^{-7}$, and Banerjea and Plattanaik $(1833.3 \pm 2.2) \times 10^{-7}$, which are all above Harrington's, and led to the revised value of e as stated above.

We have already mentioned that the ordinary form of Stokes' law proved to be insufficiently accurate for the purposes of the oil-drop method. This law connects the force X acting on a spherical particle with its velocity v of motion, its radius a , and the viscosity of the gas in which it falls as follows: $X = 6\pi\eta av$. Millikan found that he had to use a more accurate formula, taking into consideration the mean free path l of the gas, namely, $X = 6\pi\eta av (1 + Al/a)^{-1}$, where A is a numerical coefficient. This expression is usually called the Stokes-Cunningham law, although its derivation by Cunningham was spurious. On the other hand, Millikan correctly interpreted it from the very start as the result of "slipping" or "sliding friction" at the surface of the moving particle.

A technique was developed by Millikan for changing the pressure of the gas (and consequently the mean free path l) while keeping a particle in the field of observation. Thus, the oil-drop method offered a convenient means of testing the Stokes-Cunningham formula and of measuring the coefficient A . The most extensive series of measurements referred to oil droplets in air and covered the enormous range from $1/a=0.05$ to $1/a=134$. It was found that the Stokes-Cunningham law holds accurately up to $1/a=0.5$, giving an experimental value of the constant $A=0.842$.

Photoelectric Effect and Planck's Constant. The phenomena of photoelectricity were partially elucidated by the work of P. Lenard, who showed that short-wave light falling on metal makes it emit electrons. The loss of negative charge causes the metal to assume a positive potential which increases to the point where it is sufficient to make the electrons return and thus prevent the escape of even the fastest of them. There was thus a limiting upper velocity of escape and this depended on the frequency of the light. This phenomenon completely defied any explanation on classical lines and remained mysterious until Einstein introduced in 1905 the assumption of the photon constitution of light. The conception of the photon easily explained the effect and immediately led to Einstein's famous photoelectric equation, $V=(h/e)\nu+V_0$, connecting the limiting potential V with the frequency ν of the incident light and with the fundamental constants h and e ; the potential V_0 is the contact-electromotive force of the same metal when it is not illuminated.

It is true that most physicists of the time were not willing to accept this explanation, since they regarded the existence of photons as even more of a mystery than the photoelectric effect itself. But just because of the highly controversial character of Einstein's law, its experimental test was attempted by a number of independent investigators. The problem proved, however, to be technically extremely difficult. In 1913 Pohl and Pringsheim published a careful critical review of the numerous investigations and found them all inconclusive, and a similar opinion was expressed in the following year

by J. J. Thomson. The prevailing degree of uncertainty may be inferred from the fact that such a sound experimenter as C. Ramsauer came in 1914 to the conclusion that the photoelectrons have no limiting velocity at all, but are liberated with a Gaussian velocity distribution.

Millikan's ultimate success was due not only to great experimental skill but in equal measure to long experience acquired by persistent work in this sphere of phenomena. His first publications of photoelectricity were made jointly with G. Winchester and appeared as early as 1907. The purpose was to investigate whether the photoelectric current and the limiting potential depend on the temperature of the emitting metal. Such a dependence was not found—as we know today—because of the degenerate state of metal electrons. Other photoelectric work belonged to the years 1909 and 1912. All these investigations taught Millikan the importance of using very clean metal surfaces and the danger of using sparks as the source of short-wave light, since the spark discharges are liable to falsify the measured potentials by inducing in the apparatus electric oscillations. Indeed, this source of error temporarily led him astray until he corrected for it in 1913.

It does not seem that in the early period of his photoelectric work Millikan was familiar with Einstein's equation. However, when he became aware of it and directed his efforts towards testing it, his progress was rapid. The test of Einstein's equation involves the following measurements:

1. It is necessary to determine the photo-potential V . For this purpose a retarding potential is applied which stops only part of the electrons, and the current carried by the escaping electrons is measured. As the retarding potential is gradually increased, the current becomes smaller. Plotting the current against the potential, it is possible to determine by graphic extrapolation the point at which the current vanishes altogether, corresponding to the photo-potential V . The whole curve is determined point by point while the metal is illuminated with light of the constant frequency ν .

2. After the photo-potential V has been measured for a number of different frequencies, ν , it is possible to plot V against ν . According to Einstein's relation the dependence must be rectilinear: the slope of the straight line must be (h/e) and its intercept must be V_0 , corresponding to the condition $V = (h/e)\nu_0 + V_0 = 0$, whence $V_0 = -(h/e)\nu_0$. As we have mentioned above, Einstein's theory implies that V_0 is the contact-electromotive force (c.e.f.) of the non-illuminated metal. The second measurement which is necessary to test the formula is therefore the determination of the c.e.f. of the same metal surface. This determination was accomplished electrometrically. To carry out these operations on several metallic surfaces required a very elaborate piece of apparatus, in Millikan's own words: "As new operations have been called for, the tubes have by degrees become more and more complicated until it has become not inappropriate to describe the . . . experimental arrangement as a machine shop in vacuo."

The reason for Millikan's success where his predecessors failed lay in carefully choosing the conditions so as to minimize all the sources of error, of which the main were as follows: 1. The range of frequencies over which Einstein's formula had been tested in the previous work was too narrow. To extend the range Millikan used alkali metals which are photosensitive up to about $\lambda = 6000\text{\AA}$. 2. The reference bodies with respect to which the photo-potential was measured were also photosensitive, complicating the conditions by their own photo-emission due to reflected light. Millikan used as his reference body a Faraday cage of well oxidized copper netting. The photosensitivity of this material extends only to $\lambda = 2688\text{\AA}$. 3. For retarding potentials approaching the photo-potential the photo-currents became so weak that their measurement could not be made very accurately. It was found that the photo-current was many times stronger when the emitting surfaces were fresh. Hence the alkali metals were inserted into the tubes in the form of thick cylindric blocks. The "vacuum workshop" contained a rotating knife blade by means of which a thin layer of metal could be shaved off the

plane surface of the block. 4. Very troublesome were traces of stray light of higher frequency than ν . Their contribution to the photocurrent became important when this current was very small, falsifying the apparent point where electronic current approaches zero. To meet this source of error, the illumination was produced by a high pressure mercury-quartz lamp monochromized with a quartz monochromator, and the effect of stray light was further reduced with the help of proper light filters.

The results of these investigations were published in part in Millikan's own papers, in part in those of his pupils. They amounted to a complete confirmation of Einstein's equation in all its details: 1. The dependence of V on ν is rectilinear, since no experimental point was above or below the straight line by more than 1 percent. 2. The slope of the line is equal to h/e ; indeed, the experimental slopes found by Millikan were 1.376×10^{-17} for sodium, and 1.379×10^{-17} for lithium, while the best modern value is considered to be 1.3793×10^{-17} . 3. The intercept V_0 of the photoelectric straight line agreed with the electrostatically measured c.e.f. to better than 0.5 percent. These beautiful results established beyond any shadow of doubt the role which Planck's quantum of action h plays in the photoelectric effect. Besides, they represented at the time the most accurate numerical determination of that fundamental constant.

Extreme Ultraviolet Spectrum. Investigating the potentials of sparks between metallic electrodes, Millikan made, as early as 1905, the observation that the spark discharge of a large condenser could be maintained in the highest vacuum if the potential difference were sufficiently large. It occurred to him that these "hot sparks" provided a means of investigating the ultraviolet light which, in all likelihood, they were emitting. Indeed, the previous endeavors of extending the knowledge of the short ultraviolet spectrum had been limited by its extreme absorbability. But the use of a hot spark and of a concave reflection grating permitted completely eliminating all absorption by placing in a vacuum spectrograph the whole path of the rays, from their origin to the recording photographic plate. The

first results obtained with a vacuum spectrograph of this type were described by Millikan and Sawyer in 1918. The only part of this early instrument which needed improvement was the grating; through the comparison of several gratings it was found that the best result was given by gratings ruled by "the easy touch method" in which the reflecting strips were parts of the original speculum metal surface and which threw most of the diffracted radiation into the first-order spectrum. With this modification the spectrograph afforded at once a very considerable extension of the measurable ultraviolet spectrum; the region up to the line $\lambda=209\text{\AA}$ of nickel was explored right away, and up to $\lambda=136.6\text{\AA}$ of aluminum in the next following paper. All the later work on the extreme ultraviolet was done jointly with I. S. Bowen. It consisted in photographing, measuring, and completely analyzing, as to their spectroscopic terms, the spectra of numerous elements. Not only neutral atoms of the light elements emit lines in this region, but also singly or multiply ionized atoms of somewhat heavier elements. Indeed, some of the analyzed spectra were produced by atoms stripped of as many as six (S, Cl) or even seven (Cl) of their electrons.

The joint work of Millikan and Bowen opened for spectroscopy a new and fruitful region whose exploration came exactly at the right time. By supplying valuable material it influenced and helped the development of theoretical spectroscopy which at this very period was rapidly advancing towards establishing the so-called Russel-Pauli-Heisenberg-Hund rules. The number of publications by Millikan and Bowen is so large that we must restrict ourselves to a brief outline of their significance:

1. Extending the ultraviolet measurements down to $\lambda=136.6\text{\AA}$ helped to close the last unexplored gap in the spectrum of electromagnetic frequencies, because very soon F. Holweck succeeded in reaching the same wave-length region from the side of the X rays.

2. More important still, Millikan and Bowen established the essential unity of the optical and the X-ray spectra. In his presidential address of the year 1917 before the American Physical Society, Mil-

likan had already pointed out the close analogy which the X-ray spectra (Mosely's formula) bear to the hydrogen spectrum (Balmer's formula) and stressed the importance of studying the extreme ultraviolet spectra of the light elements. Carrying out this program, Millikan and Bowen showed that the s , p_2 , p_1 terms of optical series are, respectively, identical with the L_I , L_{II} , L_{III} X-ray levels.

3. The spectroscopic terms of the ultraviolet regions, like those of the visible and the X-ray spectra, were classified in terms of four quantum numbers, of which three belonged to the translational degrees of freedom of an electron, while the fourth was interpreted as residing in the atomic core. Such an interpretation involved, however, great difficulties from the point of view of the atomic model, which were forcibly stated by Bowen and Millikan in 1924 in reviewing the material they had accumulated. Precisely these difficulties caused Uhlenbeck and Goudsmit in 1925 to introduce the concept of the electron spin in order to explain the presence of the fourth quantum number mentioned above.

4. The combination of two ultraviolet spectroscopic terms occasionally gives a line in the visible spectrum. Some of these possible radiations have been identified by Bowen with unexplained lines from stellar sources and from the terrestrial atmosphere. In particular, Bowen succeeded in elucidating a series of lines observed in nebular spectra and heretofore ascribed to the hypothetical element "nebulium." He showed beyond doubt that they are due to nitrogen and oxygen by explaining the reasons why a line, which is "forbidden" and absent in terrestrial sources, may occur with considerable intensity under the conditions prevailing in a nebula. While Millikan has no direct share in these important discoveries, which are entirely due to Bowen, yet they grew out of a line of research initiated by Millikan.

Cold Emission of Metals. The phenomenon of negatively charged cold metallic surfaces giving off an electric current, when the potential gradient at their surface is very large, was first investigated by R. F. Earhart in 1901. Millikan became interested in this problem in

his early Chicago days, and it was studied in his laboratory by G. M. Hobbs in 1905. In the following years, however, his time was fully occupied with work on the electronic charge and on the photo-effect, so that he was able to return to the questions of cold emission only in the nineteen twenties. After some preliminary exploration, a thorough study was completed by Millikan and Carl F. Eyring in 1926, working with very thin tungsten wires in an extreme vacuum. They introduced for the phenomenon the term *field current* and arrived at the following conclusions: 1. Although the emission characteristics of a wire depended on its previous heat treatment, a very strong field current brought the wire into a steady state. In this state the field currents were reproducible as long as they were weaker than the field current that had been used for conditioning. 2. These field currents set in at a certain minimum potential gradient which had the order of magnitude of a few hundred thousand volts per cm. 3. The minimum potential gradients as well as the field current were entirely independent of temperature in the interval from 300° to 1000° abs. 4. The field current I seemed to be a function only of the potential gradient F at the point of emission and not to depend on the total potential difference applied to the wire. It had been claimed by other authors that $\log I$ plotted against $F^{\frac{1}{2}}$ gave a straight line; this was definitely untrue for the data found by Millikan and Eyring.

The $F^{\frac{1}{2}}$ law follows from the classical theory on the assumption that the field current is in its essence nothing but a thermionic current modified by the presence of a very strong electric field. Thus, the observations (4) of Millikan and Eyring set the cold emission apart from the thermionic emission as an independent phenomenon and their result (3) pointed in the same direction. Further work by Millikan and co-workers established that the field currents I are, indeed, quite independent of the potential difference and (within the stated limits) of the temperature and are a function of the potential gradient F only, being accurately represented by an em-

pirical formula due to Charles C. Lauritsen, $I=I_0 \exp(-b/F)$, where I_0 and b are constant.

Millikan's uncanny ability for choosing the most timely problems asserted itself also with respect to the work on cold emission. Its theoretical explanation given in 1928 independently by J. R. Oppenheimer and by R. H. Fowler and co-workers showed it to be due to the quantum-mechanical phenomenon of electrons leaking through a potential barrier. It was the first example of a previously unknown mechanism which has since received important applications in the theories of atomic and of nuclear structure. As given by Fowler and Nordheim, the theoretical law of field currents is $I=CF^2 \exp(-b/F)$, which is experimentally indistinguishable from Lauritsen's formula.

Cosmic Rays. The first reports about a penetrating radiation in the atmosphere were read before the Washington, D. C., meeting of the American Physical Society (December 31, 1902) by two independent teams of investigators: E. Rutherford and H. L. Cooke, of Montreal, and J. C. McLennan and E. F. Burton, of Toronto. At first it was believed that the origin of the penetrating radioactive rays lay in the top layers of the solid earth. However, in the years 1909 to 1911 the Swiss meteorologist, A. Gockel, took an ionization chamber (with 2-mm. brass walls) on several balloon ascents and found that the intensity of the penetrating radiation did not materially decrease up to heights 2.8 km. This result was confirmed and extended by V. Hess and W. Kolhörster, who made numerous balloon ascents up to heights of 5 km. and 9 km. respectively. With rising elevation the atmospheric ionization first decreased, reaching a minimum at about 700 m. From then on it increased, first slowly, then more rapidly. This pointed to a component of the penetrating radiation coming downward from high altitudes, possibly from outside the terrestrial atmosphere, whence the name "cosmic rays." Though this result was in principle well-established by 1915, the quantitative side of the measurements was by no means accurate. Indeed, estimates of the coefficient of absorption of the downward

radiation, made on the basis of Kolhörster's data, lay in the vicinity of $\mu = 0.57 \times 10^{-2} \text{ cm}^{-1}$ of water, a value which later proved to be greatly in error.

Millikan became actively interested in cosmic rays, following his removal to Pasadena, when he conceived the idea of sending self-recording instruments high up into the atmosphere with the help of sounding balloons. The first recording electroscopes and barometers were constructed by him jointly with I. S. Bowen, and in the spring of 1922 sent up to heights of 15.5 km. The same year he caused his student, R. M. Otis, to make cosmic-ray measurements on Mt. Whitney (4,130 m.), and the following summer he went with Otis to the top of Pike's Peak (4,300 m.) for an elaborate series of measurements. Although this early work did not compare in accuracy with his later standards, yet it proved conclusively that the coefficient of absorption derived from Kolhörster's data was far too high. This discrepancy disappeared when Kolhörster, after measuring cosmic-ray intensities in ice caves of the Jungfrau glacier, scaled down his absorption coefficient to less than one-half, namely, to $\mu = 0.25 \times 10^{-2} \text{ cm}^{-1}$ of water. Thus it became evident that the cosmic rays are many times more penetrating than any known radioactive rays, a fact which clearly demonstrated the importance of their further investigation. While heretofore cosmic rays had been studied only by meteorologists and specialists in radioactivity, Millikan recognized in them a subject capable of yielding information of wider importance for the whole of physics.

From then on Millikan brought to bear on this problem the whole of his vast experience as an experimental physicist. His next piece of work was carried out jointly with G. H. Cameron, and consisted in sinking electroscopes to various depths of mountain lakes and in measuring the cosmic-ray intensity as a function of depth. Chosen were Muir Lake (3,540 m.), near the top of Mt. Whitney, and Lake Arrowhead (1,530 m.) in Southern California; the greatest depth to which the instruments were lowered was 27 m. The very neat absorption curves which their measurements yielded justify the state-

ment that it marked the beginning of modern accuracy in cosmic-ray work. Two important results were inferred from the analysis of these curves: 1. It was concluded that all the penetrating radiation came from above the upper lake and, within the precision of the analysis, no part of it had its origin in the air between the levels of the two lakes. 2. The radiation was found to have a band structure consisting of harder and softer components whose coefficients of absorption ranged from $\mu=0.30$ to $\mu=0.18$. Later, as the measurements were extended to higher elevations and to greater depths in water, new, softer, and harder components were found, results which were confirmed by other observers. Millikan realized fairly early that the radiation measured in the atmosphere is not necessarily the primary radiation coming from outside but may consist of secondary and tertiary rays. Hence it is not safe to make inferences about the nature of the primary cosmic radiation from the band structure observed in one single geographical location.

The subsequent cosmic-ray work of Millikan and his collaborators was far too comprehensive to be surveyed paper by paper. We shall restrict ourselves to enumerating its most significant features:

1. For many years the electroscopes developed by Millikan and his school were more accurate than those employed by other workers. Marked improvement in accuracy was achieved by the use of high pressure ionization chambers, with 8 atmos. pressure of air in 1928 and 30 atmos. in 1931. (Subsequently the air was replaced by argon.) Two years later H. V. Neher developed his self-recording instrument with a very sensitive, temperature-independent, and vibration-free quartz system which even today satisfies all requirements of precision.

2. Because of the superior accuracy of his measurements, Millikan was able to disprove claims put forward, at different times, by other observers with respect to large daily variations of cosmic-ray intensity in dependence on the positions of the sun and of the stars. His school contended from 1923 on that the diurnal variations are either very small or nonexistent.

3. For the same reason Millikan and co-workers obtained, comparatively early, good curves of cosmic-ray intensities at high elevations in the atmosphere. Airplane flights yielded accurate results from 1933 on; sounding balloons, whose data were at first less reproducible, gradually also became highly reliable. It was found that the intensity rises with altitude to a certain height, then reaches a maximum and declines. Inasmuch as the response of the ionization chamber stands in a simple relation to the energy of the radiation, it is possible to derive from an intensity curve the total energy penetrating from outside into the atmosphere in the form of cosmic rays. Thus Millikan's energy determinations are independent of any hypothesis about the nature or mechanism of the radiation phenomena.

4. After the geomagnetic effect had been discovered by J. Clay and co-workers, Millikan and Neher found that it was more strongly marked at high altitudes. On the one hand, it was possible to calculate the velocity of the primaries eliminated by the geomagnetic effect between two locations of different latitudes. On the other hand, the comparison of the atmospheric cosmic-ray-intensity curves for these two locations yielded the coefficients of absorption of the radiation components weakened or removed in passing from the first location to the second. The results showed that the primitive view of cosmic-ray absorption as entirely caused by ionization was quite untenable. Since the existence of the maximum in the curves, mentioned under (3), pointed in the same direction (as also the existence of some unusually penetrating cosmic-ray corpuscles, ascertained by other observers with Geiger counters), these data formed a strong incentive for the development of modern theories of the stopping of fast particles.

5. In 1929 D. Skobelzyn presented strong evidence that some fog tracks he had observed in a Wilson expansion chamber were caused by cosmic rays. Sensing with his characteristic intuition the opening of a new research province, Millikan realized at once that here was a new and promising approach to the problems of the nature of cosmic rays and of the mechanism of their absorption. He con-

structed in 1931, jointly with Carl D. Anderson, a large vertical expansion chamber in a homogeneous magnetic field of 20,000 gauss. Even the early photographs showed the presence of electrons with kinetic energies of over 10^9 electron volts. It is common knowledge how in Anderson's hands (seconded by S. H. Neddermeyer) this method led to the discovery of the positron and the meson and how it continues to yield insight into atomic phenomena of fundamental importance in nuclear physics.

6. In 1942 Millikan, in collaboration with Professors H. V. Neher and W. H. Pickering, engaged in a refined analysis of the primary cosmic-rays spectrum by means of studying the geomagnetic effect. As a result of extensive investigations it was concluded that the energy spectrum of the primaries possesses a band structure. To explain both the origin of cosmic rays and the existence of the bands, Millikan proposed the theory of annihilation of atoms in the interstellar space. He suggested that atoms get annihilated in a single elementary process, converting their whole intrinsic energy into the kinetic energy of a positive-negative particle pair created in the process. This theory of atom annihilation was later disproved, but its value as a working hypothesis could not be denied.

In any case, the pioneering work of Millikan and his colleagues in the Norman Bridge Laboratory opened up the field and led dozens of other laboratories all over the world to initiate studies in this field. From these studies emerged the modern concept of the primary cosmic rays as consisting largely of very high energy atomic nuclei plunging into our atmosphere from outer space, causing cataclysmic nuclear disruptions as they collide with atoms in the earth's atmosphere. From these events emerge gamma rays, electrons, positrons, neutrons, protons, mesons of various kinds, and the whole range of "strange particles" which were constituting such a puzzle to the theoretical physicists in 1958. And in 1958 Millikan's successors in the Norman Bridge Laboratory were still taking a leading part in slowly unraveling the mysteries.

Millikan's scientific activities covered many more subjects than the

ones outlined above. In his biography in *American Men of Science* he listed twenty-one research subjects in which he had worked. He might have listed many more in which he guided and counseled his students and colleagues in the fields of X rays, thermionic emission, physics of solids, nuclear physics and others. In all these fields of physics he displayed that uncanny intuition for picking the most basic and most timely problems. He could hardly have foreseen in 1925 that the study of cosmic rays would yield results of such surpassing importance in the field of nuclear physics. So unpromising indeed did it appear that he and his associates had the field almost to themselves for several years until its great importance had become evident. He himself was the first to admit—even to proclaim—that Lady Luck often smiled on him and his work. But it was Millikan himself who could capitalize on the slightest smile and turn it into a major advance in science.

CONCLUSION

There is no use denying that Millikan had stormy days in his varied activities. His many vigorous debates on the nature and origin of cosmic rays were major events in the world of science in the 1930s. His determination to elevate the prestige of Caltech as rapidly as possible led to his being dubbed one of the great publicity agents in the field of education. He often differed vigorously with his colleagues on matters of politics, philosophy, and religion. His administrative methods were hardly conventional, often confusing, yet his strong personal leadership always pulled things back into shape.

A long associate of his has said, "The secret of his success lay to a large extent in the simple virtues instilled in his upbringing. He had a single minded devotion to all that he was doing, and he put his work above his personal desires and aspirations. His combination of native good sense and intellectual honesty led him far both in science and in public life. In spite of his success and high public position, he always remained a simple man of true humility. He was not always,

technically speaking, a 'good administrator,' but even this worked to the advantage of Caltech: he never lost sight of the ultimate aims; he was the rare administrator for whom the interests of science came first.

"But, above all, he was a man with a warm heart, a kind benevolent man always eager to be helpful, even at the cost of trouble and inconvenience."

In short, he combined a rare insight into science with a rare understanding of human beings.

KEY TO ABBREVIATIONS

- Ann. Phys.=Annalen der Physik
 Ann. Phys. Chem.=Annals of Physical Chemistry
 Astrophys. J.=Astrophysical Journal
 Deut. Phys. Gesell. Verhandl.=Deutsche Physikalische Gesellschaft Verhandlungen
 Ind. Eng. Chem.=Industrial and Engineering Chemistry
 Int. Cong. Arts Sci.=International Congress of Arts and Sciences
 J. Am. Med. Assoc.=Journal of the American Medical Association
 J. Chem. Ind.=Journal of Chemical Industry
 J. Franklin Inst.=Journal of the Franklin Institute
 Phil. Mag.=Philosophical Magazine
 Phys. Rev.=Physical Review
 Phys. Zeits.=Physikalische Zeitschrift
 Pop. Sci. Mo.=Popular Science Monthly
 Proc. Am. Phil. Soc.=Proceedings of the American Philosophical Society
 Proc. Inst. Radio Eng.=Proceedings of the Institute of Radio Engineers
 Proc. Nat. Acad. Sci.=Proceedings of the National Academy of Sciences
 Pub. Astro. Soc. Pacific=Publications of the Astronomical Society of the Pacific
 Rev. Modern Phys.=Reviews of Modern Physics
 Sci. Mo.=Scientific Monthly
 Trans. Am. Electrochem. Soc.=Transactions of the American Electrochemical Society
 Trans. Chem. Soc.=Transactions of the Chemical Society (London)

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T H Morgan

THOMAS HUNT MORGAN

September 25, 1866–December 4, 1945

BY A. H. STURTEVANT

THOMAS HUNT MORGAN was born September 25, 1866, at Lexington, Kentucky, the son of Charlton Hunt Morgan and Ellen Key (Howard) Morgan.

In 1636 the two brothers James Morgan and Miles Morgan came to Boston from Wales. Thomas Hunt Morgan's line derives from James; from Miles descended J. Pierpont Morgan. While the relationship here is remote, geneticists will recognize that a common Y chromosome is indicated. The family lived in New England—mostly in Connecticut—until about 1800, when Gideon Morgan moved to Tennessee. His son, Luther, later settled at Huntsville, Alabama. This Luther Morgan was the grandfather of Charlton Hunt Morgan; the latter's mother (Thomas Hunt Morgan's grandmother) was Henrietta Hunt, of Lexington, whose father, John Wesley Hunt, came from Trenton, New Jersey, and was one of the early settlers at Lexington, where he became a hemp manufacturer. Ellen Key Howard was from an old aristocratic family of Baltimore, Maryland. Her two grandfathers were John Eager Howard (Colonel in the Revolutionary Army, Governor of Maryland from 1788 to 1791) and Francis Scott Key (author of "The Star-spangled Banner"). Thomas Hunt Morgan's parents were related, apparently as third cousins. Francis Scott Key's mother was Ann Phoebe Penn Dagworthy Charlton, daughter of Arthur Charlton of Frederick, Maryland; Henrietta Hunt's maternal grandmother was Mary Charlton—who seems to have been a sister of Ann.

The pedigree indicates that the greatest portion of Morgan's ancestry was of English origin, but besides that of the immigrant James Morgan there are at least two other Welsh strains, a number of surnames that seem to be Scotch, and at least one infusion each of Irish, French, and German—a mixture similar to that usually found in the ancestry of Americans whose families have been long in this country.

Charlton Hunt Morgan was American Consul at Messina in 1860, and at that time assisted Garibaldi—a circumstance long remembered by Italian patriots, with the result that his son felt especially close to Italy, and was received there with great cordiality. Shortly after his return, in 1861, Charlton joined the Confederate Army, being a member of the group known as "Morgan's Raiders," that was commanded by his brother, General John Hunt Morgan. Charlton was wounded at the battle of Shiloh, and was several times captured.

Thomas Hunt Morgan was interested in natural history as a boy. Some of his summers were spent in the mountains at Oakland, in extreme western Maryland, where he collected fossils. As a young man he was employed for several summers in geological and biological field work in the Kentucky mountains. In 1886 he received the B.S. degree at the University of Kentucky, and then proceeded to Johns Hopkins University, having first attended the marine laboratory at Annisquam, Massachusetts, during the summer of 1886. His own account of how he came to go to Johns Hopkins is as follows: "My days at Johns Hopkins were probably not very different from those of other students who were attracted there by the rather vague rumors that reached us as undergraduate students in far distant colleges. In my own case it was through Joseph Castle who had preceded me by a couple of years. Perhaps the fact that my mother's family were Baltimoreans had some effect; but little did I know then how little they appreciated that a great university had started in their midst, and I think this was typical of most of the old families of that delightful city" (from a letter written about 1943).

He always felt that he owed much to the atmosphere at Johns

Hopkins under Daniel Coit Gilman—and the similar one he encountered later at Bryn Mawr under President M. Carey Thomas.

At Johns Hopkins Morgan was a student of William Keith Brooks, and it was Brooks who influenced him in his choice of embryology as his first field of study. To Brooks also must be attributed the encouragement of his long interest in marine organisms. However, he was also greatly influenced, in his student days, by H. Newell Martin and by W. H. Howell. From them he learned to appreciate the value of physiological approaches to biology; and I think he was inclined to turn to them rather than to Brooks at times because he felt that the latter was somewhat too metaphysical for his tastes.

Among his fellow students at Hopkins were E. G. Conklin and R. G. Harrison—who remained among his closest friends for the rest of his life. Also in the Hopkins tradition, although he had already left there before Morgan arrived, was E. B. Wilson, later to be Morgan's close associate and personal friend. These three men were, I think, the ones among his contemporaries with whom he felt the closest ties.

Morgan received his Ph.D. from Johns Hopkins in 1890, and was then Bruce Fellow for a year—part of this time being spent in research in Jamaica and the Bahamas. In 1891 he was appointed Associate Professor of Biology at Bryn Mawr, succeeding E. B. Wilson. He remained at Bryn Mawr until 1904. Among his associates here were R. G. Harrison and J. Loeb, with both of whom he maintained close associations for many years thereafter. Here also he had a number of students who made great contributions to biology; especially to be noted are Nettie M. Stevens, whose work in regeneration and particularly in cytology was outstanding, and Lilian V. Sampson, whose early work was in embryology and regeneration, and to whom Morgan was married in 1904. In 1904 Morgan was appointed Professor of Experimental Zoology at Columbia, a position he held until 1928. During this period he was closely associated with E. B. Wilson. They were a remarkable pair, and those of us who were graduate students under them can appreciate the atmosphere of the labora-

tory more easily than we can describe it. Harrison (1936. *Science*, n.s., 84:565), writing about Wilson, says: ". . . Wilhelm Ostwald, in his interesting book on great men of science, classified them, according to their talents, as romantics and classics. . . . To the romantic, ideas come thick and fast; they must find quick expression. His first care is to get a problem off his hands to make room for the next. The classic is more concerned with the perfection of his product, with setting his ideas in the proper relation to each other and to the main body of science. His impulse is to work over his subject so exhaustively and perfectly that no contemporary is able to improve upon it. . . . It is the romantic that revolutionizes, while the classic builds from the ground up.

"Wilson is a striking example of a classic, and it is interesting to note that for many years his nearest colleague and closest friend was an equally distinguished romantic."

The two men did not always agree on scientific questions, but the disagreements were openly discussed and each respected the other's opinions. All this was apparent to the students in the department—and gave them an example of the scientific spirit at its best.

In 1928 Morgan was appointed Professor of Biology at the California Institute of Technology, a position he held until his death. His undertaking here was to organize a Division of Biology—a subject not then taught at the Institute. He preferred research to administrative work, but this seemed to him an opportunity not to be missed, for he was dissatisfied with the way in which biology was still so largely dominated by morphology in most institutions; here he had a free hand to develop the subject in the way he wanted, in an institution where physics and chemistry were outstanding, and where the primary emphasis was on research and the training of research men. He felt—and the event justified this feeling—that he could count on the support, in this undertaking, of the three guiding spirits of the California Institute: George Ellery Hale, R. A. Millikan, and Arthur A. Noyes.

Morgan was always interested in marine biology. As indicated

above, the summer before he began graduate work was spent at the marine laboratory at Annisquam. Two years later (in 1888) he spent his first summer at Woods Hole, where he continued to work nearly every summer for the remainder of his life. Here he made many friends, and played a large part in the scientific activities of the several laboratories situated there. He was made a Trustee of the Marine Biological Laboratory in 1897; he took an active and constructive part in the development of that laboratory from that time on.

Ten months in 1894 and 1895, and also the summer of 1900, were spent at the Zoological Station at Naples, which he had visited in 1890. Here he collaborated with Hans Driesch in the use of experimental methods in the study of embryology. This association was important in influencing the course of his later work. He found Driesch congenial and stimulating, and remained on close friendly terms with him—even though he found himself wholly out of sympathy with Driesch's later vitalistic views and preoccupation with philosophy.

After moving to California in 1928, Morgan continued to go to Woods Hole in the summers, but he also started a marine laboratory at Corona del Mar as an integral part of the new Division of Biology of the California Institute of Technology. Here, for the first time, work with marine forms became for him an all-year occupation.

Morgan's first work was in descriptive embryology, directed (as was the fashion at the time) toward the solution of phylogenetic problems. His doctoral dissertation, on the embryology of the Pycnogonida (sea spiders), and work on the embryology of *Balanoglossus*, belong to this period. Early work on the development of Amphibia and ascidians was also descriptive, but directed rather toward determining the exact history of specific embryonic regions. Later embryological work was almost entirely experimental in nature; this interest is apparent in his report of work done on the eggs of fish and of sea urchins and starfish in 1893. However, it was in the summer of 1894, at Naples, that this interest was strongly reinforced,

largely by his association with Driesch. The early experiments here were concerned with the determination of the median plane of the embryo, with the old problem of "concrecence" in the vertebrate embryo, and with the development of isolated blastomeres. In one of these papers (1894, *Anat. Anz.*) he reported the effects of fertilizing fragmented eggs of sea urchins, and reached the conclusion that Boveri had been mistaken in supposing that non-nucleated fragments could be fertilized with foreign sperm and would then develop and give rise to embryos like those of the paternal species. There resulted a controversy which was ended only by a posthumous paper by Boveri (1918, *Arch. Entw. Mech. Org.*, 44) in which he finally concluded that Morgan had been correct in his interpretation. It may be argued that this controversy was unfortunate, for it was largely responsible for Morgan's relative lack of appreciation for Boveri's later brilliant work. This was one of the respects in which Wilson was a useful counterpoise for graduate students at Columbia, for his admiration of Boveri was infectious.

In 1896 Morgan reported on the induction of artificial asters in sea urchin eggs by the use of hypertonic sea water. This was an outgrowth of earlier observations by the Hertwigs, Loeb, and Morgan himself. It was important in connection with Boveri's theory of the self-duplicating nature of the centrioles, and in the developments that led to Loeb's work on artificial parthenogenesis. There was at the time a rather general feeling that Loeb had taken more credit than was due him for the discovery of artificial parthenogenesis. A study of the literature of the period suggests that in fact the idea was "in the air," and that it was only a question of who would first find a technique that would lead to reasonably normal cleavage of the treated eggs. In later years Morgan sometimes talked about this matter; he clearly felt that Loeb had been secretive about his own work and had used every opportunity to find out just what Morgan was doing. However, Morgan was not as resentful as were some other members of the Woods Hole group on his behalf, and he and Loeb

were on close friendly terms during the period when I knew them—from 1913 until Loeb's death in 1924.

While he was still a graduate student Morgan had begun experiments on the regeneration of earthworms, and in 1897 he published the first of a long series of papers on regeneration in a wide variety of animals—planarians, Crustacea, coelenterates, teleost fishes, Amphibia, etc. At the time of his death he was studying regeneration in brittle stars. His book on the subject (1901), like some of his other books, was not intended as a summary of a well understood field. I once heard him say, semi-seriously, that the only book worth writing was one in a field that was developing so rapidly that the book would soon be out of date.

The work on experimental embryology and regeneration was directed largely toward the problem of differentiation: how does a relatively undifferentiated egg or tissue come to produce the orderly and regulated series of successive structural complications that lead to the fully formed adult? Here again there was a difference in attitude between Morgan and Wilson. The latter was inclined to lay great emphasis on the segregation of preformed materials in the egg during cleavage, whereas Morgan had little faith in the effectiveness of "formative stuff." His experiments with Lyon on the effects of centrifuging eggs (1907) represented an attempt to test this hypothesis experimentally.

Morgan was interested in the determination of sex as early as 1903, when he published a critical review of the literature. This was followed by a suggested interpretation of gynandromorphism in the honey bee (1905; the correctness of the interpretation was finally shown in 1951, by Rothenbuhler, Gowen, and Park, *Genetics*, 36:573). Then, beginning in 1906, came his work on the complicated life-cycle of the phylloxerans and aphids. He showed that the facts, which at first seemed quite inconsistent with the chromosome interpretation of sex determination, were in fact intelligible only in terms of that interpretation. This was one of Morgan's most brilliant achievements, involving great skill and patience in the collecting and

care of the animals, insight in seeing what were the critical points to study, and ability to recognize and to follow up unexpected facts. The results were of importance in serving to demonstrate the role of the chromosomes in sex determination, at a time when that importance was seriously questioned by many biologists.

Morgan's interest in genetics seems to have stemmed, at least in large part, from a visit to de Vries's garden in Holland (probably in 1900). In 1903 he wrote "No one can see his experimental garden, as I have had the opportunity of doing, without being greatly impressed." What impressed him was the occurrence of numerous sharply distinct types, differing from the parental forms in numerous respects, and breeding true to the new characteristics. It is, of course, now known that these types arise because of the very special and peculiar genetic properties of *Oenothera*, and that they do not furnish evidence for the kind of conclusions that were drawn by de Vries. But to Morgan they seemed to offer a means of escape from what seemed to him the sterile and somewhat teleological speculations of the extreme advocates of natural selection. This remained with him in later years; he could be persuaded that selection can in fact operate through the sorting out of numerous modifying genes, and that there is nothing mysterious or teleological about the process of natural selection—but it was always a point of view with which he was basically dissatisfied.

Morgan began work on *Drosophila* in an attempt to induce mutations; but before he took up that material he had already begun his strictly genetic work, using mice (beginning in 1908) and rats (1909). He was at first quite critical of parts of the Mendelian interpretation, beginning in 1905 with his questioning of the "purity of the germ cells"—i.e., of the principle of segregation. As late as 1909 he wrote: "In the modern interpretation of Mendelism, facts are being transformed into factors at a rapid rate." His argument here was that the interpretation was "preformationist," whereas he felt that an "epigenetic" interpretation was more hopeful. The point seems to have been that he felt the Mendelian factors to be arbitrary

inventions, made up to fit the facts, with no independent evidence for their existence, and not capable of experimental demonstration. One may suspect that he was influenced by his negative reaction to the highly speculative scheme of heredity developed by Weismann. It was characteristic of the man that these reservations about the reality of the genes as discrete and sharply separable units were quickly and completely discarded as soon as he became more familiar with the experimental results.

Morgan's first paper on *Drosophila* appeared in 1910. *Drosophila* seems to have been bred as a laboratory animal for the first time by C. M. Woodworth, who was later Professor of Entomology at the University of California. In the academic year 1900-1901 Woodworth was a student at Harvard University. He had cultures of *Drosophila* breeding in the laboratory, and called the attention of W. E. Castle to its availability as a convenient object for breeding experiments. Castle began experiments on the effects of inbreeding, ultimately published in 1906 (Castle, Carpenter, Clark, Mast, and Barrows, *Proc. Amer. Acad. Arts and Sci.*, 41:729-86). Meanwhile the material had been used for behavioral studies in the Harvard laboratory by Carpenter, whose 1905 paper (*Amer. Nat.*, 39:157-71) marks the beginning of the published experimental work on *Drosophila*.

It has usually been stated that Morgan got his original cultures of *Drosophila* from F. E. Lutz, who was at the Carnegie Laboratory at Cold Spring Harbor from 1904 to 1909, and later at the American Museum of Natural History. Lutz worked with *Drosophila* at least as early as 1907; and both he and Morgan have indicated in print that Morgan got his original material from him. However, Dr. F. Payne (in a personal communication) questions this. Payne was a graduate student at Columbia from 1907 to 1909, and during that time undertook experiments that involved breeding *Drosophila* in the dark (published 1910 and 1911, *Biol. Bull.*, vols. 18 and 21). These experiments were begun in October, 1907. He reports that Morgan was very insistent that he collect the beginning strains him-

self by exposing fermenting fruit, rather than by getting cultures from Lutz. It may be noted that Quackenbush also bred *Drosophila* in the Columbia laboratory, and that it appears from his paper (1910, *Science*, n.s., 32:183) that his strains stemmed from collections made in 1908 at Woods Hole. It may also be pointed out that it was evidently common knowledge at this time that *Drosophila* could readily be collected from decaying fruit and used to start laboratory cultures. Moenkhaus (1911, *Jour. Morphol.*, 22) used material that he had himself collected in Indiana, and he stated that these experiments were begun in 1903. (It may be noted that Payne had taken his A.B. and A.M. degrees at Indiana University, where Moenkhaus was working, before he came to Columbia.) Stevens (1910, *Jour. Exper. Zool.*, 5), in her account of the chromosomes of *Drosophila melanogaster* (then known as *D. ampelophila*) stated that she had bred the species in the Bryn Mawr laboratory in the winter of 1906–1907, but did not state how she obtained the material. Lutz (1911, *Carnegie Inst. Washington*, Publ. 143) stated that his experiments were begun at least as early as 1907, with wild-caught material. His reports in the *Carnegie Year Books* suggest that the work was in fact begun in 1907.

It is not clear when Morgan himself began work with *Drosophila*. We have seen that Payne, in his laboratory, began in 1907 and kept cultures going into 1909, and that Quackenbush worked with cultures established in the summer of 1908. Morgan's first paper on the material was read before the Society for Experimental Biology and Medicine on May 18, 1910. This first paper reports on the original white-eyed mutant and the F_1 obtained from him; in July, 1910, appeared the paper demonstrating the sex-linked inheritance of white eyes—the first of the major discoveries made with *Drosophila*. The July paper says that the original white-eyed male appeared “in a pedigree culture of *Drosophila* which had been running for nearly a year.” This shows that Morgan had been rearing *Drosophila* at least since the summer of 1909.

Morgan stated several times (in personal communications) that he

began work with *Drosophila* in the hope of inducing mutations. He used "wide ranges of temperature, salts, sugars, acids, alkalis," and radium and X rays. It is now known that the two latter should have been effective, but the techniques for the detection of mutations were inadequate, and the few that were found after exposure to radium were attributed to chance rather than to the treatment. Mutations were found, but the conclusion was that they were not induced by the treatments used. It appears from the first paper mentioned above that selection (for a dark thoracic pattern) was also carried on in the very early experiments.

The cytological studies on the sex-chromosomes, leading to the discovery of the X-chromosome mechanism of sex determination (suggested by McClung in 1901, corrected and demonstrated by Stevens and by Wilson in 1905), had indicated the male as the heterozygous sex in several orders of insects (Orthoptera, Heteroptera, Homoptera, Coleoptera, Diptera); whereas the genetic data on sex-linkage (beginning with Doncaster in 1906) had indicated the female as the heterozygous sex in Lepidoptera and also in canaries and in fowls. Both results were thus known in enough forms to suggest that they were generally applicable—and yet they were flatly contradictory. It was against this background that Morgan's discovery of sex-linkage in *Drosophila* was made in 1910. Here was a case, in every way similar to those known in birds and moths—but here the male was the heterozygous sex, and this was in a group in which Stevens had already shown that the male was the heterozygous sex as judged by the chromosome picture.¹ This was a major step in the development of the chromosome theory of inheritance—

¹ It was apparent from the published pedigrees that color-blindness in man "follows the same scheme as does white eyes in my flies" (Morgan, *Amer. Nat.*, Feb. 1911, p. 77). This apparently was the first definite specification of sex-linkage of the *Drosophila* type in man, although an equally brief passage by Wilson (June, 1911, *Arch. mikr. Anat.*, 77:249) has recently been cited. It seems clear that both Morgan and Wilson understood the point. It is now scarcely possible to decide who should be given priority—and one may be certain that neither of them was concerned about priority at the time.

and in a few months Morgan followed it with another major step.

New mutant types kept appearing in his cultures, and a second sex-linked one (now known as "rudimentary") was soon found. Here, then, were two separate mutant types, one with white eyes and the other with rudimentary wings, and according to the chromosome theory each of the genes concerned should be in the X chromosome. On crossing the two strains and rearing an F_2 generation, Morgan found that recombination occurred in the F_1 female that was heterozygous for both genes. As he saw, this indicated that there was some sort of exchange of parts between homologous chromosomes. There had been speculations that this might occur, and Janssens had presented cytological evidence indicating to him that it did in fact occur. This cytological evidence was not conclusive, and the idea was not generally accepted—although it was becoming clear that only in some such way as this could the chromosome interpretation of Mendelian inheritance be saved. This crucial experiment of Morgan's (reported at a meeting of the Society for Experimental Biology and Medicine on October 19, 1910) was thus a great step forward.

White and rudimentary happen to lie far apart in the X chromosome, with the result that it was not apparent in this first cross that they were linked—i.e., that they did not segregate independently in the doubly heterozygous female. But in 1911 cases of linkage had been recognized—most obvious in the relation between yellow body and white eyes—and Morgan then laid down the essence of the modern chromosome theory of heredity. The basis of linkage is nearness together in the chromosomes, and recombination between linked genes is due to exchange of parts between homologous chromosomes in some such way as that suggested by Janssens.

In the winter of 1910-1911 Morgan took C. B. Bridges and the writer—both then undergraduates—into his laboratory, and gave us desks in what came to be known as the "fly-room." This was a rather small room, with eight desks crowded into it, in which the three of us reared *Drosophila* for the next seventeen years. There was a steady stream of other students using the room also—rarely were

there fewer than five people working there at any one time. The post-doctoral foreign students who came to Columbia to work on *Drosophila* were regularly given desks here—e.g., O. L. Mohr, H. Nachtsheim, C. Stern, G. Bonnier, T. Komai, E. Gabritschewsky, T. Olbrycht, A. Zulueta, Y. Imai, T. Dobzhansky. In addition there were, at one time or another, F. N. Duncan, E. Cattell (later Mrs. H. J. Bagg), Mrs. T. H. Morgan, E. Altenburg, J. S. Dexter, A. Weinstein, J. W. Gowen, D. E. Lancefield, and E. G. Anderson. Others, who did not have desks in the “fly-room,” but worked actively with the group and were often in and out, are too numerous to mention individually—but among them H. J. Muller must be especially indicated, since his share in the early developments was especially important.

This group worked as a unit. Each carried on his own experiments, but each knew exactly what the others were doing, and each new result was freely discussed. There was little attention paid to priority or to the source of new ideas or new interpretations. What mattered was to get ahead with the work. There was much to be done; there were many new ideas to be tested, and many new experimental techniques to be developed. There can have been few times and places in scientific laboratories with such an atmosphere of excitement and with such a record of sustained enthusiasm. This was due in large part to Morgan’s own attitude, compounded of enthusiasm combined with a strong critical sense, generosity, open-mindedness, and a remarkable sense of humor. No small part of the success of the undertaking was due also to Wilson’s unfailing support and appreciation of the work—a matter of importance partly because he was head of the department.

Because of the close cooperation in the work it is very difficult to trace the individual contributions to the developments in this period.² But in 1915 there appeared *The Mechanism of Mendelian Heredity*,

² This statement is not to be taken as applying to the conclusions described above as having been reached by 1911. These were certainly due to Morgan and not to the newly formed group.

by Morgan, Sturtevant, Muller, and Bridges. This was the first serious attempt to interpret the whole field of genetics in terms of the chromosome theory. It was a landmark in the history of the subject, for all significant later developments have taken that theory for granted.

In 1915 Morgan obtained a grant from the Carnegie Institution of Washington for the support of the *Drosophila* work; this grant was continued until his death. Part of it was used to support the maintenance of the living stocks of mutant types, which had become—and still remains—a time-consuming job requiring careful and technically trained workers. The rest of the grant was used to support Bridges and the writer (and, later, J. Schultz) as full-time assistants. What this meant was that we had research positions. Morgan did not direct our work. This was characteristic; he did his own work, and had no desire to develop a group working under his direction. The same attitude was evident in his relations to graduate students; he rarely assigned problems—the student was expected to find his own problems—and he did not lay out the approach to problems in any detail, though he was always ready to talk to a student about his work. We referred to Morgan as “The Boss;” but this was a term of affection and respect, not at all intended to imply that he was a taskmaster.

During the Columbia period the whole group regularly moved to Woods Hole each summer—except for the summers of 1920 and 1921, and the intervening academic year, which were spent in California (at the Hopkins Marine Station, Pacific Grove, at Stanford University, and at the University of California, Berkeley). The *Drosophila* cultures were shipped in barrels, and experiments were never interrupted for these summer moves. Morgan always had other experiments under way—with chickens, pigeons, mice, rats, or plants—and this material was carried by hand on the Fall River Line. At Woods Hole, Morgan himself always carried on some experiments with marine organisms, even while he was actively working with *Drosophila*.

During Morgan's later years at Columbia his own experiments with *Drosophila* gradually decreased in number, and after the move to Pasadena in 1928 they practically ceased. He remained very active in the discussions of the work, and in his support and encouragement of the rest of the group. But his own work—which never ceased—came to be more and more with other material and other problems.

To this period belongs a series of other types of work. The earlier studies of experimental embryology and of self-sterility in *Ciona* were continued at Woods Hole, and later also at Corona del Mar. In regeneration there was the work on fiddler crabs, which resulted in a solution of the problem of why the males have one large claw and a smaller mate, the "right- and left-handed" specimens being equally numerous. Here also came the experiments on the inheritance of the number of tail feathers in pigeons, and the important studies on the inheritance of hen-feathering in fowls and the effects of castration on hen-feathering. There were also numerous projects that did not yield published results. Morgan at one time or another collected and studied numerous strains of the plants *Coleus*, *Verbena*, and *Calochortus*. He also studied the Bidder's organ of the toad, the seasonally developed secondary sexual characters of the salamander *Triturus*, regeneration of the arms of brittle stars, crosses between different geographical races of deer mice, and rapid changes in color of the living "goldbug" (*Coptocyclus*), and other questions with a wide variety of organisms.

Morgan was a good naturalist. He knew a surprising number of animals, and knew how and where to collect them, and he was remarkably skillful at keeping them alive and in good condition in the laboratory. I have made up a list of the various forms on which he published results. There are over fifty kinds of animals and one plant (burdock) on the list, which reads in part as follows: Protozoa (*Stentor*), coelenterates (*Gonionemus*, *Tubularia*, ctenophores, etc.), flatworms (both land and fresh-water), nemerteans, annelids (earthworms and marine forms), gephyreans, Crustacea (crabs,

crayfish), insects (phylloxerans and aphids, *Drosophila*), pycnogonids, mollusks (gastropods and bivalves), echinoderms (sea urchins, sand dollars, starfish), ascidians, *Balanoglossus*, *Amphioxus*, teleost fishes, salamanders, frogs, toads, birds (pigeons, peafowl, chickens, phalarope), mammals (mice, rats).

He was inclined to use simple techniques and equipment. The early *Drosophila* cultures were reared in a miscellaneous assortment of milk bottles, that were accumulated by various more or less unorthodox methods. It was customary to put a piece of paper in each culture bottle, and Morgan was very likely to use for this purpose the envelopes in which his current correspondence came. The flies were examined with hand lenses. He was aware of the necessity of elaborate and expensive equipment in much of modern experimental biology; but it was always somewhat difficult for him to support requests for such equipment. This was in line with his usual policy in money matters; when his own pocket was involved he was very generous, and many a student was helped financially by him (though he did not like this to be known); but he was very saving—sometimes it seemed almost miserly—when the source was institutional. He always tried to stay as far as he could under any budget that he administered, and was reluctant to ask for an increased budget.

Morgan was the recipient of many honors and awards, including the Darwin Medal (1924) and the Copley Medal (1939) of the Royal Society, and the Nobel Prize in Medicine (1933). He was a member of many scientific societies, including the Royal Society (Foreign Member), and the American Philosophical Society. He served as president of the following: American Morphological Society (1900), American Society of Naturalists (1909), Society for Experimental Biology and Medicine (1910–1912), National Academy of Sciences (1927–1931), American Association for the Advancement of Science (1930), Sixth International Genetics Congress (1932).

Morgan was married in 1904 to Lilian V. Sampson, who survived him. There were four children, who also survived him: Howard K.

Morgan, Mrs. Douglas Whitaker, Mrs. Henry W. Scherp, and Dr. Isabel M. Morgan (now Mrs. Joseph D. Mountain). He died at Pasadena, California, on December 4, 1945.

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KEY TO ABBREVIATIONS

- Acad. Nat. Sci. Phila. Jour.=Journal of the Academy of Natural Sciences of Philadelphia
- Amer. Breeders' Assn. Rep.=American Breeders' Association Reports
- Amer. Jour. Physiol.=American Journal of Physiology
- Amer. Jour. Psychol.=American Journal of Psychology
- Amer. Med.=American Medicine
- Amer. Med. Assn. Jour.=Journal of the American Medical Association
- Amer. Nat.=The American Naturalist
- Amer. Phil. Soc. Proc.=Proceedings of the American Philosophical Society
- Anat. Anz.=Anatomischer Anzeiger
- Anat. Rec.=The Anatomical Record
- Arch. Entw. Mech. Org.=Archiv für Entwicklungsmechanik der Organismen
- Arch. Zellforsch.=Archiv für Zellforschung
- Bibliogr. Genet.=Bibliographia Genetica
- Biol. Bull.=Biological Bulletin
- Biol. Zentralblatt=Biologisches Zentralblatt
- Calif. Inst. Tech. Bull.=Bulletin of the California Institute of Technology
- Carnegie Inst. Wash. Year Book=Carnegie Institution of Washington Year Book
- Columbia Univ. Biol. Ser.=Columbia University Biological Series
- Encyc. Brit.=Encyclopaedia Britannica
- Ent. Soc. Amer. Annals=Annals Entomological Society of America
- Franklin Inst. Jour.=The Journal of the Franklin Institute
- Harper's Mag.=Harper's Magazine
- Johns Hopkins Univ. Circ.=Johns Hopkins University Circulars
- Johns Hopkins Univ. Stud. Biol. Lab.=Studies from the Biological Laboratory of the Johns Hopkins University
- Jour. Exp. Zool.=The Journal of Experimental Zoölogy
- Jour. Gen. Physiol.=The Journal of General Physiology
- Jour. Hered.=The Journal of Heredity
- Jour. Morph.=Journal of Morphology
- Jour. Phil. Psych. Sci. Meth.=The Journal of Philosophy, Psychology and Scientific Methods
- Linnean Soc. Lond. Proc.=Proceedings of the Linnean Society of London
- Med. News=Medical News
- Nat. Acad. Sci. Biog. Mem.=National Academy of Sciences Biographical Memoirs
- Nat. Acad. Sci. Proc.=Proceedings of the National Academy of Sciences
- N. Y. Acad. Sci. Annals=Annals of the New York Academy of Sciences
- Physiol. Rev.=Physiological Reviews
- Pop. Sci. Mo.=Popular Science Monthly

- Quart. Jour. Micr. Sci.=The Quarterly Journal of Microscopical Science
 Quart. Rev. Biol.=The Quarterly Review of Biology
 Roy. Soc. Lond. Obit. Not.=The Royal Society of London, Obituary Notices
 Roy. Soc. Lond. Proc.=Proceedings of the Royal Society of London
 Sci. Mo.=The Scientific Monthly
 Smithsonian Inst. Ann. Rep.=Smithsonian Institution Annual Report
 Soc. Exp. Biol. Med. Proc.=Proceedings of the Society for Experimental Biology and Medicine
 Torrey Bot. Club Bull.=Bulletin of the Torrey Botanical Club
 Yale Rev.=The Yale Review
 Zeit. indukt. Abstamm.-u. Vererblehre.=Zeitschrift für induktive Abstammungs-und Vererbungslehre
 Zool. Anz.=Zoologischen Anzeiger
 Zool. Bull.=Zoological Bulletin

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FREDERICK GEORGE NOVY

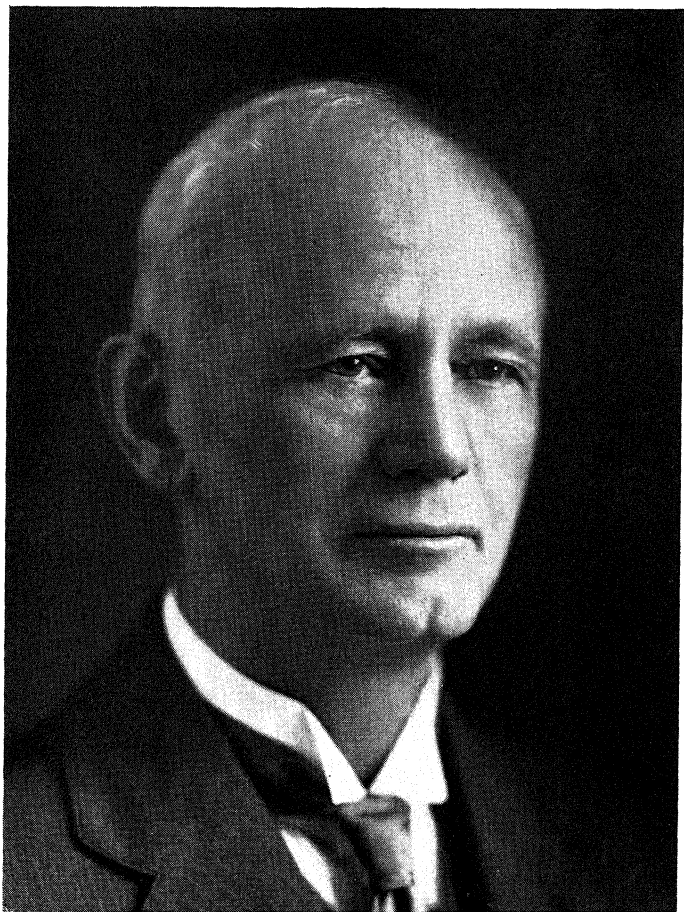
December 9, 1864–August 8, 1957

BY ESMOND R. LONG

FREDERICK GEORGE NOVY, pioneer teacher and investigator of bacteriology, died in Ann Arbor on August 8, 1957, at the age of ninety-two years and eight months. He was among the last survivors of that eager group of young American biologists and hygienists who journeyed to Europe in the 1880s to work in the laboratories of Louis Pasteur and Robert Koch and learn the fundamentals of the new science of bacteriology at first hand. In the succeeding seventy years Novy saw almost the entire development of modern microbiology as an academic discipline and field of research.

Novy was born in Chicago on December 9, 1864, the third son of Bohemian parents who had moved to America earlier that year. Novy's father had been a master tailor in the old country, and continued the occupation in this country. His mother had been a milliner; a heritage of property, which she sold, made possible their trip to America and establishment of a new home. Each of them had attended a village school in Bohemia; each had read intensively and had developed a desire to travel. Some light on Frederick Novy's longevity is seen in their long lives; his father died at eighty-four and his mother at eighty-three.

The first family home in Chicago was on the West Side, not far from the site of origin of the great Chicago fire of 1871, which Novy recalled vividly the rest of his life. Novy attended the public schools in Chicago after a year at the "Mission School," a primitive struc-



J. G. Henry

ture at the corner of Halsted and 12th Streets. He was studious, even in his early years, with a strong interest in history and a rapidly developing devotion to science. At the West Division High School he was stimulated greatly by his chemistry teacher, Marc de la Fontaine, an independent investigator as well as teacher. Like so many other boys who later became eminent in biological fields, the young Novy was fascinated by the opportunities of his environment and constantly sought ways of improving his understanding of it. De la Fontaine had shown him how to use a microscope, and nothing would satisfy Novy until he had an instrument of his own. He worked nights in the Chicago Public Library to secure the necessary money, and after he acquired the coveted tool he began, with intense enthusiasm, to search ponds and fields near and far for life invisible to the naked eye. His interest in microscopy was permanent; in later years it led directly into the bacteriology he did so much to develop. It is worthy of note that some sixty years later, at a meeting of the American Association for the Advancement of Science, he was awarded the 250,000th microscope manufactured by the Bausch and Lomb Company.

At the time, however, his study of microscopic life was an avocation; his real purpose was to follow in the footsteps of his teacher, de la Fontaine, and be a chemist. De la Fontaine was an admirer of Albert B. Prescott, Professor of Organic Chemistry at the University of Michigan, and to the latter Novy wrote for information on courses in chemistry at that university. So impressed was Novy by Prescott's replies that he became eager to enter the University of Michigan. His extraordinarily understanding and receptive parents met this wish by moving to Ann Arbor and starting life again in that city. There young Novy could pursue his college course while living at home. Ann Arbor remained his home for seventy-five years.

Novy was graduated from the University of Michigan, after specializing in chemistry, in 1886. He looked briefly for a job elsewhere but immediately accepted an offer from his teacher Dr. Prescott of a modest post in the university's Department of Organic Chemistry.

He began work immediately for a master's degree and attained this in June, 1887, with a thesis on cocaine and its derivatives. He followed up his first paper with a few additional short publications on cocaine and related compounds. These investigations were in the field of pure organic chemistry and represented exactly the type of professional effort that had attracted Novy to Ann Arbor in the first place. It was a short-lived occupation, however, for in the year in which he took his master's degree in chemistry Victor C. Vaughan, Professor of Hygiene and Physiological Chemistry in the university, persuaded him to take the position of instructor in his department.

Thus began a long association, marked by close collaboration and numerous joint publications, of which the best known was their book *Cellular Toxins* (1902; see appended bibliography), which started out as a smaller work in 1888, *Ptomaines and Leucomaines, or the Putrefactive and Physiological Alkaloids*. There can be no doubt that Novy's original interest in pure organic chemistry laid the background for these studies in bacteriological chemistry and developed a lasting sense of precision in his investigations. He retained that interest throughout life. He continued his membership in the Deutsche Chemische Gesellschaft, which he acquired in 1886, for nearly thirty years, relinquishing it during the First World War. His talent in chemistry led to his selection as a member of the Council on Pharmacy and Chemistry of the American Medical Association, a position he held for a quarter of a century (1905-1930).

A student always, Novy followed a graduate and medical curriculum as rapidly as he could in the midst of his academic responsibilities and received the Sc.D. degree in 1890 and the M.D. degree in 1891. In the latter year he was made Assistant Professor of Hygiene and Physiological Chemistry under Vaughan. The latter had just become Dean of the Medical School, so that much of the professional responsibility for the department devolved on Novy. This early obligation, imposed when he was less than thirty years of age, threw him into the midst of a highly stimulating group, vividly concerned with the development of medical instruction in the Mid-

dle West. The group included J. Playfair McMurrich and G. Carl Huber in anatomy, Henry Sewall and Warren P. Lombard in physiology, Arthur R. Cushny in pharmacology, Aldred Scott Warthin in pathology, and George Dock in medicine.

Shortly after the beginning of his association with Vaughan, young Novy, influenced by the rising tide of reports on the agents of communicable disease and already conscious of the coming importance of bacteriology for students of medicine, suggested to Vaughan that they use vacation time for a trip to Europe to learn facts and new methods of study in the rapidly developing science. Novy had been particularly impressed by Ferdinand Hueppe's *Die Methoden der Bakterien-Forschung*, which had just been published, and first suggested that they study with Hueppe in Wiesbaden. Vaughan acquiesced heartily in the idea of the trip, but proposed that they make their first objective the famous laboratory of Robert Koch in the Hygienic Institute in Berlin. This met with Novy's approval. In Berlin Novy worked primarily under Koch's assistant Carl Fraenkel, but listened to lectures by Koch himself. Novy wrote a short but picturesque account of the daily routine in Fraenkel's student laboratory, and its methods of intensive instruction, shortly after his return to the United States.

Back in Ann Arbor in the fall, he made preparations for a course in bacteriology, which was officially offered as a three-month elective in the university in January, 1889. Relatively few students took the course, but some of the faculty attended. Among these was the Professor of Physiology, Henry Sewall, a victim of tuberculosis, who wished to study his own case by the new bacteriological technics. Sewall was one of the first to appreciate Novy's talents and foresight. So successful was the course that in the fall of the next year, 1890, bacteriology became a requirement for students in the medical curriculum.

Novy made several subsequent trips to Europe to improve his understanding and acquaintance. He studied at the famous University of Prague, always a leading center for pathology, in 1894, and

at the Pasteur Institute in Paris in 1897, two years after Pasteur's death. He had, as a matter of fact, visited the Institute on his return from Berlin in 1888, but apparently without making the actual acquaintance of Pasteur. Although he missed an association with the French master, in subsequent years Novy knew the Pasteur Institute well and cultivated a warm and lasting friendship with an early successor of Pasteur, Emile Roux, one of the discoverers of diphtheria toxin. In his 1894 trip, again in company with Vaughan, he attended the International Congress of Hygiene and Demography held in Budapest that year, making the trip as a delegate from the Michigan State Board of Health. At this meeting he had the unique experience of hearing Alexandre Yersin describe his discovery of the plague bacillus and Roux and Emil von Behring report independently on their pioneer investigations on diphtheria antitoxin.

In America Novy grew rapidly in stature as teacher and investigator. By 1900 his place as a foremost bacteriologist in this country was well established. He had published a textbook for students, collaborated with Vaughan in a widely read volume on ptomaines, taken part in local studies on typhoid fever, written authoritatively on the hog cholera bacillus, discovered one of the several etiological agents of malignant edema, developed apparatus and methods for investigating the biology of anaerobic bacteria, and written extensively on laboratory methods in general bacteriology.

In 1900, when a report from an officer of the U. S. Marine Hospital Service raised the suspicion that bubonic plague was smoldering in San Francisco's Chinatown, Novy's reputation was such that he was a logical choice for membership in a commission appointed by the Secretary of the Treasury—whose responsibilities then included what is now the Public Health Service—to ascertain if the disease really was rife. The commission, consisting of Simon Flexner of Philadelphia, Chairman, Llewellys F. Barker of Chicago, and Novy, located several cases of bubonic plague, proving conclusively that the disease was endemic, and opened the way for rigid measures for the prevention of its spread. Novy used his experience and the exhaustive back-

ground he accumulated through concurrent studies of plague to write instructional articles for both the medical and lay public on the subject and to indicate what provisions should be adopted if the disease escaped its confines and spread to other parts of the United States.

In the same year, 1900, Novy took part in the foundation of the Society of American Bacteriologists; in 1904 he became its fifth president, succeeding Theobald Smith in that influential position. Two years previously, in 1902, he had been made full Professor and head of the now well-established Department of Bacteriology in the University of Michigan, after passing through the lower academic grades in the Department of Hygiene and Physiological Chemistry.

At the time when Novy commenced his work in bacteriology and related microbiology, relatively little original work in the field had been done in this country. Theobald Smith and his associates had discovered the cause and mode of transmission of Texas fever in cattle. George M. Sternberg, an early investigator in many fields of bacteriology, a systematist and authoritative teacher, was one of the several discoverers of the pneumococcus and had written a *Manual of Bacteriology* which was a leading text in the field in this country for years. William H. Welch and T. Mitchell Prudden had brought German knowledge and methods to the United States and were training many young men who later assumed posts of importance. As a result of this and other stimulation, in the late eighteen eighties and early nineties embryo courses and investigative laboratories for bacteriology were appearing throughout the country. The course given by Novy in 1889 is commonly described as the first formal university course in the country. The practical applications of bacteriology were by that time widely recognized, however. It was rapidly taking its place as an indispensable auxiliary science in pathology and public health and had made its way into the teaching of medicine and surgery.

At the turn of the century numerous American journals were devoting all or much of their space to bacteriological developments;

these included the *Journal of Medical Research* (founded as the *Boston Journal of the Medical Sciences* in 1896), the *Journal of Experimental Medicine*, also founded in 1896, and the *Journal of Infectious Diseases*, founded in 1904. The *American Journal of Public Health*, which went through a series of transitions in name and format before its present stabilization, was from its earliest years a medium for publication of the applications of bacteriology to public health.

Novy was an early entrant in the field of bacteriological instruction. In 1894 he put out a short informal text on *Laboratory Work in Bacteriology*, which was elaborated, revised, and enlarged in a second edition in 1899. It came out through Novy's insistence, as noted in the preface of the book, that a thorough course of laboratory instruction in bacteriology was essential in the proper education of medical students. The text was based on the day-to-day work of the Hygienic Laboratory of the University of Michigan and represented the painstaking teaching that characterized all of Novy's efforts.

Although Novy began his professional life, as we have seen, as a chemist and published a chemical text for medical students (*Directions for Laboratory Work in Urine Analysis*, 1892, entitled *Laboratory Work in Physiological Chemistry* in its second edition, in 1898), his early responsibilities in hygiene and public health, fostered by Victor Vaughan, and his own self-acquired training in microscopic zoology led him inevitably into a career in microbiology. His association with Vaughan and his own interest in chemistry rapidly carried him on to a search for toxic products of bacterial growth. In these early studies, and partly under the influence of Vaughan, Novy was convinced that within the bodies of microorganisms, or among the products of their metabolism, substances of definable composition could be discovered, whose action could be correlated with the symptoms of disease. This was the view of a number of German bacteriological chemists, particularly Ludwig Brieger of Berlin, and so it is not surprising that Novy's discovery of certain free bases in the bacterial products of the hog cholera bacillus led him to identify

them tentatively as specifically active products in hog cholera. His more mature considerations of a protein product in the microorganism, against which some immunological response could be built up, was more in line with modern thinking.

Investigations of much greater importance, carried out some years later and published in 1894, led to his discovery and isolation of an anaerobic bacillus causing fatal septicemia in animals, with characteristic "malignant edema." Such organisms had been known as far back as the researches of Pasteur in the 1870s and August Gaertner of Jena identified a similar organism, which was soon designated generally as Gaertner's bacillus. In the succeeding years Novy's organism came to be recognized as a significant one, distinguishable from Gaertner's, and clearly one of the several agents capable of causing malignant edema.

The study of this and similar organisms led Novy into ingenious developments of apparatus for the cultivation of anaerobic bacteria. These investigations were reflected in equipment invented years later for studies on bacterial respiration. In the meantime the "Novy jar" had become standard bacteriological equipment. It was but one of many useful pieces of apparatus he devised during his period of intensive laboratory investigations. So well-recognized were his accomplishments in bacteriological technic that he was chosen for responsibility in this field in the authoritative statements published from time to time in the *Reference Handbook of the Medical Sciences*.

His activities were extra- as well as intramural, and he proved an expert in the now deeply cultivated field of public relations. Throughout the 1890s he was vigorously engaged in acquainting the Michigan public with facts on the germ theory of disease, on the nature of diphtheria, typhoid fever, and tuberculosis, on food poisoning, on disinfection, and on many similar matters of significance for public health. He maintained close relations with the Department of Health of the state of Michigan all his life; his steadfast

service and his own achievements were recognized by the Michigan legislature by a special testimonial in 1931.

Shortly after the turn of the century he began the work for which he is best known, the study of trypanosomes and spirochetes. The investigations on spirochetes were published a little later than those on trypanosomes, but the studies were closely related, and for convenience the spirochetal studies will be considered first. So intense was Novy's devotion to this field, and so well known in the medical school were his studies that he acquired the nickname "Spi," and Spi Novy he remained to students and friends the rest of his life.

Among his first investigations of spirochetes were studies directed toward the etiological agents of relapsing fevers, of which the best known was the *Spirocheta obermeieri* described by Otto Obermeier about 1870. One natural habitat of the organism was the blood of the rat, and it was customary to maintain strains of the spirochetes in the laboratory by successive transfers to rats; attempts at artificial cultivation in the laboratory had consistently failed. Novy and his associate, R. E. Knapp, succeeded in cultivating the rat spirochetes in blood-filled collodion sacs in the rat peritoneal cavity and thus broke the long stalemate in cultivation methods. Their investigations, supplementing those of others, made it clear that several different strains of spirochetes were concerned in the relapsing fevers of different geographic regions. The strain with which Novy and Knapp did most of their work, which had been received from a colleague in New York, later came to be known as *Spirocheta novyi*.

The investigations furnished the basis for a controversy with the distinguished German protozoologist Fritz Schaudinn, the discoverer of the microbic cause of syphilis. Schaudinn believed in an interrelationship of spirochetes and trypanosomes, the former being considered a transitional stage in the life cycle. Novy's studies showed that they were independent forms of life, and Schaudinn admitted the fact in later years.

Opinions differ on what were Novy's most fruitful contributions to science. To many of his colleagues his work on trypanosomes has

had the most influence. His first object in the field was the development of methods for their artificial cultivation. In this, in association with Ward J. MacNeal, he was eminently successful. For their investigations they used strains of the common rat trypanosome, *T. lewisi* and the virulent organism *T. brucei*, the etiological agent of nagana, or the tsetse fly disease of South Africa. Novy and MacNeal devised a procedure for growing the trypanosomes in pure culture in the condensation water of slanted tubes of blood agar. This appears to have been the first cultivation of a pathogenic protozoon.

Their results aroused prompt interest and early confirmation from abroad. The distinguished Charles Nicolle, Director of the Pasteur Institute of Tunis, introduced a modification of their blood agar medium which became known as the "N:N:N medium" (after Novy, MacNeal, and Nicolle, on the mistaken assumption abroad that MacNeal's last name was Neal).

Characteristically Novy carried his investigations into the field of artificial stimulation of resistance to trypanosome infections. He and his associates found that they could build up some immunity. An important outgrowth of the studies was a deep interest in the problem of anaphylaxis. Novy and Paul H. de Kruif of the Department of Bacteriology, with others in the department, including two of Novy's sons, Robert L. Novy and Frank O. Novy, followed up certain observations made in the course of their efforts to immunize animals against the virulent trypanosomes of nagana (*T. brucei*) and surra (*T. evansi*). They noted an extraordinary drop in temperature and acute collapse in some of the inoculated animals, and chronic intoxication and ultimate fatality in others. It appeared that some specific poison was being introduced, or induced in formation, in the blood. The postulated poison, as far as could be deduced from the symptoms, apparently had something in common with the anaphylatoxin hypothesized by others as an explanation of the symptoms of anaphylaxis, which had been a puzzle ever since the phenomenon had been brought to the attention of the medical world by the French physiologist Charles Richet.

The speed of the acute reaction in the inoculated animals, and the minuteness of amount of trypanosomal poison capable of eliciting it, convinced Novy that the toxic substance was of extraordinary potency. The finding that it, or a toxic substance of similar potentiality, could be generated by mere contact of rat serum with the trypanosomal antigen, appeared to furnish a lead for determining its real nature. It was soon learned that incubation of the serum with other colloids, particularly comminuted agar, resulted in a similar development of toxicity. This increased the lines of attack but complicated the problem, since a characteristic feature of anaphylaxis is its high degree of specificity.

Chemical studies indicated that anaphylatoxin production was not the result of protein cleavage, but some reason was uncovered to indicate that changes of the fibrinogen of the blood were concerned. Additional studies, carried out by others of Novy's staff, indicated that the alkali reserve of the blood was not modified. No firm conclusions were reached, but studies by de Kruif and other associates linked the phenomenon of anaphylaxis with the globulin fraction of the serum and particularly the euglobulin. The essential nature of the potent substance remained undisclosed, however. In the meantime, investigators in many other laboratories were baffled in a similar way, and the general trend seemed to be to await a ripper time for solution of the anaphylatoxin problem.

A later generation at the University of Michigan looked upon Novy's studies in microbic respiration as the climax of his work. These investigations, carried out in the mid 1920s when Novy was in his early sixties, formed another of the several well conceived and broadly executed programs conducted by Novy and other members of his staff. Novy's genius in devising and adapting apparatus was instrumental in the precision of attack on the problem. As in the case of the trypanosomes and anaphylatoxin, a lengthy series of publications resulted.

Certain of the pathogenic protozoa, including *Trypanosoma lewisi*, with which he had had signal success in the past, were among

the microorganisms used in the study. The best-known results, however, were those secured in association with Malcolm H. Soule, on the respiration of tubercle bacilli. Their studies gave a clear picture of all of the important features of gas exchange during the growth of tubercle bacilli, and indicated that the slow growth of these organisms in the body could be explained on the basis of respiration and multiplication under conditions of low oxygen tension. Today such studies are carried out with standardized equipment, such as the Warburg apparatus, with which minute amounts of oxygen and carbon dioxide can be measured, but no greater accuracy in uncovering the major aspects of growth is achieved than was attained by Novy and Soule with the more cumbersome but effective devices of Novy and his associates.

Novy's last scientific article, which appeared in 1953 when Novy was nearly ninety years of age, was one with a storybook background. More than forty years previously he had developed an interest in ultramicroscopic life and filterable agents of disease, i.e., the viruses which occupy the attention of a large proportion of all microbiologists today. In the course of his studies with other members of his department on trypanosomes, Novy had obtained evidence of a filterable agent accompanying the trypanosomes in rats' blood and responsible for an encephalitis that accompanied the trypanosomal manifestations of disease on inoculation of the blood in experimental animals. Much was learned of the nature of the agent, but all work had to be stopped when the stock material containing the virus was unaccountably lost. Fortunately the records of the experiments were meticulously kept. Thirty-five years later, during the cleaning of an old laboratory preparatory to a move, workers found a set of tubes of dried blood, which were promptly identified as the long missing virus culture. Appropriate tests showed that, in spite of long storage and desiccation, the original virus in the tubes, although attenuated, was still viable. The original notes were brought out and the significant data were published, Novy being as excited and eager as any one of the active staff. Students of Novy

(Walter J. Nungester and Harry E. Morton), casting up the total on known viruses, calculated that this virus was actually among the earliest discovered, a feat ensuring Novy a place among the pioneer virologists.

Novy's students and associates have left abundant testimony to his unique talents as investigator and teacher. His insistence on adequate control in research, and his industry and logic in teaching, earned for him a reputation as a strict disciplinarian. It is commonly accepted that Novy was the original model for the scientifically uncompromising Dr. Gottlieb in Sinclair Lewis' *Arrowsmith*, although Novy himself could not reconcile the picture of Dr. Gottlieb's guttural, broken English with his own facility of expression and polished style, of which he was not a little proud. But his role as a taskmaster was tempered. Novy's many students, who are carrying on with the precepts he taught, remember his humor when they recall his mannerisms and always note the generous interest behind his sharp comments. His biographer, Ruth Good, wrote that although he was a master of the barbed tongue his wit was on the kindly side and often used to lessen the pain of his caustic criticisms.

In the late years of his academic life, as with so many men who have proven their organizing ability by success in research, increasing administrative responsibilities were thrust upon him. His scientific investigations, of course, were correspondingly curtailed. From 1930 to 1933 he was intensively occupied with duties as chairman of the Executive Committee of the Medical School. In 1933 he was elected Dean of the school. It was a perfectly natural course for a teacher and investigator with his broad perspective in medical education. He had written much on the subject in its widest aspects, and had an intimate understanding of the necessities in keeping up with the progress of medical science. He continued as Dean, as well as head of the Department of Bacteriology, until he reached the age of retirement in 1935.

Many honors were conferred on Novy, some of which have already been noted in this memoir. He was elected to the National

Academy of Sciences in 1924 and to the American Philosophical Society in 1934. He was elected to the Association of American Physicians at the early date of 1900. He was Henry L. Russell Lecturer at the University of Michigan in 1927 and George M. Kober Lecturer at Georgetown University in 1931. His name was selected for enduring distinction in the Welch-Novy-Russell lectures of the Society of American Bacteriologists. He was gold medalist of the American Medical Association in 1930. In 1931 he received a citation from the Legislature of the State of Michigan for his long and tireless service to the state's Department of Health. Abroad he was made a Chevalier of the Legion d'Honneur and a member of the Order of the White Lion of Czecho-Slovakia, the native land of his parents.

He was a constituent member of numerous societies and, in the course of his professional life, president of many of them. Among his presidencies were those of the Society of American Bacteriologists (1904), the American Society for Experimental Pathology (1921), and the American Association of Immunologists (1924). He served as vice-president for the Section on Pathology and Bacteriology in the International Congress on Tuberculosis in Washington in 1908, and for the section on tropical medicine in the International Congress on Hygiene and Demography in Washington in 1912. He was a vice-president of the International Congress on Microbiology in London in 1936 and honorary president of this Congress in New York in 1939. He was a charter member of the American Academy of Tropical Medicine and its vice-president in 1936. He held a series of administrative offices in the scientific and medical societies of his own state of Michigan. His interest in medical student affairs led to a long and close association with the medical fraternity Nu Sigma Nu, of which he was honorary president in 1932 and secretary of the corporation for the long period 1897 to 1947. Many societies graced their own records by electing him an honorary member. He was a life member of the American Associa-

tion for the Advancement of Science and one of its vice-presidents in 1910.

A nonagenarian in 1954, and mentally alert if not physically active, Novy had many years to look back upon the science he had helped create. He considered himself an old-time bacteriologist and always listed himself as such. The fields of his greatest achievement were not strictly in bacteriology, however, but rather in the broader domain of microbiology. As we have seen, his work encompassed several of its recognized major branches: true bacteriology, protozoology, virology, and immunology. In his fifty years of intense professional activity he had seen remarkable developments in understanding in all of these fields. In the twenty-two years of retirement, he remained a recognized authority and wise counselor, whose advice and assistance were constantly in demand.

ACKNOWLEDGMENTS

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KEY TO ABBREVIATIONS

- Amer. Chem. J.=American Chemical Journal
Amer. J. Med. Sci.=American Journal of the Medical Sciences
Amer. J. Roentg. Rad. Therap.=American Journal of Roentgenology and Radium Therapy
Amer. Med.=American Medicine
Ann. Int. Med.=Annals of Internal Medicine
Ann. Rep. Surg. Gen. U.S.M.H.S.=Annual Report of the Surgeon General of the United States Marine Hospital Service
Bol. Asoc. Med. Puerto Rico=Boletin de la Asociacion Médica de Puerto Rico
Centralbl. Bakt.=Centralblatt für Bakteriologie und Parasitenkunde
J.A.M.A.=Journal of the American Medical Association
J. Anal. Chem.=Journal of Analytical Chemistry
J. Appl. Micros.=Journal of Applied Microscopy
J. Exper. Med.=Journal of Experimental Medicine
J. Hyg.=Journal of Hygiene
J. Infect. Dis.=Journal of Infectious Diseases
J. Lab. Clin. Med.=Journal of Laboratory and Clinical Medicine
J. Mich. State Med. Soc.=Journal of the Michigan State Medical Society
Med. News=Medical News
Med. Surg. Reporter=Medical and Surgical Reporter
Mich. Alumnus=Michigan Alumnus
Pharm. Era=Pharmaceutical Era
Pharm. Rundsch.=Pharmaceutische Rundschau
Proc. Amer. Assn. Adv. Sci.=Proceedings of the American Association for the Advancement of Science
Proc. Ann. Conf. Health Officers=Proceedings of Annual Conference of Health Officers in Michigan
Proc. Soc. Exper. Biol. Med.=Proceedings of the Society for Experimental Biology and Medicine
Rep. Counc. Pharm. Chem.=Reports of the Council on Pharmacy and Chemistry of the American Medical Association
Rep. Mich. Acad. Sci.=Report of the Michigan Academy of Science
Rep. State Board Health=Report, Secretary of the State Board of Health, Michigan
Trans. Assn. Amer. Physicians=Transactions of the Association of American Physicians
Trans. Clin. Soc. Univ. Mich.=Transactions of the Clinical Society of the University of Michigan
Trans. Internat. Congr. Hyg. Demog.=Transactions of the International Congress on Hygiene and Demography

Trans. Mich. State Med. Soc.=Transactions of the Michigan State Medical Society

Trans. Pan-Amer. Med. Congr.=Transactions of the Pan-American Medical Congress

Ztschr. Hyg.=Zeitschrift für Hygiene und Infektionskrankheiten

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G. W. Pierce

GEORGE WASHINGTON PIERCE

January 11, 1872–August 25, 1956

BY FREDERICK A. SAUNDERS AND FREDERICK V. HUNT

GEORGE WASHINGTON PIERCE, physicist, engineer, teacher, and inventor, died in retirement at his summer home in Franklin, New Hampshire, on August 25, 1956. He was born on January 11, 1872, on a farm in Webberville, Texas, a small rural community in Travis County just east of the state capital at Austin. His mother was Mary Gill Pierce, the second wife of George W. Pierce (senior), a farmer, cattleman, and butcher. He had one older half-brother, James Byron Pierce, and a younger brother, Ben Colvin Pierce. The latter was named, after a common custom of that day, for the notorious gambler and one-time marshal of Austin, Ben Thompson—a singular coincidence, since George himself was to occupy the chair of physics at Harvard endowed by the earlier and in some ways equally notorious Benjamin Thompson who later became Count Rumford.

Growing up on a farm often cultivates the traits of self-reliance and ingenuity, but it can also be a rugged discipline, especially under the semi-frontier conditions that prevailed during the reconstruction period in central Texas. It did not take long for Pierce to acquire a cordial dislike for farm chores, and he often remarked in later years that the frustrations of hauling water for thirsty mules from a deep well with a leaky pail provided a powerful incentive for pursuing higher education elsewhere.

Pierce began his studies in the local one-room school, where he soon discovered that it was possible to do his own assignments and

at the same time to absorb the more advanced studies being taught at the front of the room to the older children. His academic horizons were broadened a little when his family moved to the neighboring town of Taylor, and his initiative and keen intelligence promptly gained him his first academic kudos, the inaugural award of a thin triangular gold medal for high honors in the fifth grade of the newly established public school in Taylor.

Pierce became an avid reader, encouraged in this perhaps by the discovery that he could avoid some farm chores by pleading preoccupation with homework. He managed to pick up a working knowledge of Spanish from itinerant Mexican laborers, and he later reported of his early schooling that he "liked especially English, mathematics, and history; and disliked nothing." As a result, and because he had "bootlegged" the study of algebra on his own, he was given advanced-standing credit in English, mathematics, physics, and chemistry when he entered the seven-year-old University of Texas in 1890 at the age of eighteen. Two fellow students of that time still living in Austin remember him as a brilliant and advanced student, and his work in physics and mathematics was sufficiently outstanding to gain him exemption from many final examinations and to earn him the B.Sc. degree in three years. In fact, there is reason to think that his teachers may have had trouble at times in keeping one jump ahead of him, and he probably remembered this in later years when he enjoyed pretending that his own students were outdistancing him. His physics professor, Dr. Alexander Macfarlane, chose him as a research assistant during his senior year, and their work led to Pierce's first publication, a joint paper with Macfarlane that appeared in the first volume of the *Physical Review* in 1893. An extension of this work during the following year earned him his master's degree and formed the subject matter of his first solo publication.

Hard times were widespread in the middle 1890s and the world was not then crying out for young physicists. After leaving the University of Texas in 1894, Pierce spent four years earning a precarious living by tutoring, doing odd jobs, "assisting an oculist in

investigations of the eye," and by teaching science in the Austin High School (1895), mathematics and modern languages in Hargrove College (Ardmore, Indian Territory, 1895-96), and natural sciences in the Dallas High School (1896-97). He also worked for nearly a year in the clerk's office of the Bastrop County Court, where he acquired a knack with legal phraseology and a feeling for due process that served him well in later years; and he took advantage of the opportunity afforded during the summer of 1897 to collaborate with Professor E. F. Northrup, briefly attached to the University of Texas, in "Some Observations on the Dielectric Strength of Oils" and in "Some Experiments in Induction with Currents of High Frequency at Long Distances." In a footnote to the latter joint publication, Northrup remarked that a statement by Professor J. Trowbridge of Harvard suggested a further experiment that might be made "with an instrument of the character described"; from which it may be inferred that Northrup's advice is likely to have been influential in steering Pierce toward Harvard. In any case, the most significant by-product of this four-year period of latency seems to have been the conviction it gave Pierce that his real love was scientific research and that a career in this field would have to be pursued elsewhere.

Pierce had an inexhaustible supply of salty anecdotes and apocryphal tales which were the constant delight of his associates. One of the most often repeated of these was the story he told of how he happened to come to Harvard in 1898 instead of going to the University of Chicago. His meager resources included no allowance for train fare, so he made his way to the railroad junction at St. Louis by working as chief wrangler on a cattle train. There he discovered that the only train for Chicago on which he could work his way during the next few days was a sheep train, whereas a cattle train was leaving at once for the east coast. For one raised in the Texas cattle country this was a simple choice, and Harvard gained a new and eager scientist.

Nothing is lost from the foregoing tale if it be confessed that

Pierce had already applied to Harvard in the spring of 1898 for scholarship aid, and that the Whiting Fellowship had been awarded to him for study in the Physics Department for the academic year 1898-1899. Miscellaneous odd jobs augmented this modest support, and one of them revealed the characteristic Pierce touch. A group of Cuban students arrived for the Harvard Summer School and asked for a guide to help them see the beauties of New England. Pierce applied for the job and went to President Eliot for an interview. When asked for his qualifications, he replied that he could speak Spanish and that he was experienced in loading cattle on trains. He got the job.

The challenge Pierce was seeking was generously provided by his adviser, Professor John Trowbridge, and by such other Harvard "greats" as Wallace Clement Sabine, Edwin H. Hall, B. O. Peirce, and W. E. Byerly. He picked up another master's degree in 1899, and took his Ph.D. in 1900 with a thesis topic of his own choosing: "The Application of the Radio-Micrometer to the Measurement of Short Electric Waves." Professor Trowbridge took a dim view at the outset of his selection of this research problem, but the three publications that grew out of the work soon convinced him that Pierce had good judgment as well as high ability.

The award to Pierce of a John Tyndall Scholarship for 1900-1901 enabled him to spend his first post-doctoral year abroad, most of it in Leipzig working with Boltzmann. Once again his targets were of his own choosing, and out of this work came the material for two articles on the indices of refraction and the phenomena of double refraction for electric waves, topics he investigated with the aid of an improved form of the radio-micrometer he had devised two years earlier for his doctoral research. His nontechnical visits to Berlin and other parts of Germany, and a holiday bicycling junket to Italy, strengthened his command of European languages and heightened his interest in Germanic literature, as was evidenced by his choice of subject matter for the avocational poetry he wrote at this time—and was courageous enough to preserve.

In 1901-1902 Pierce was back at Harvard as an assistant in physics, starting his climb up the academic ladder. He was appointed instructor in 1902, assistant professor in 1907, and Professor of Physics in 1917; and in 1921 he succeeded E. H. Hall to become the seventh Rumford Professor of Physics. He was given in 1935 the additional title of Gordon McKay Professor of Communication Engineering, which thereby further enhanced the academic dignity of a technical subject he had a large share in creating. He served also as the Director of the Cruft Memorial High Tension Electrical Laboratory from the completion of its construction in 1914, and as Chairman of the Division of Physical Sciences from 1927, until his retirement as Professor Emeritus in 1940.

Pierce's curiosity about the behavior of electric waves had by no means been satisfied by his doctoral research and his studies abroad, and the growing technical interest in "wireless" telegraphy drew him like a magnet toward this field. Our present sophistication concerning electric circuitry makes it difficult to appreciate the primitive state of knowledge about resonance and the tuning relations in coupled circuits which prevailed during the first decade of this century. Pierce made a frontal attack on these problems and published between 1904 and 1907 a definitive series of five papers dealing with both theoretical and experimental aspects of "Resonance in Wireless Telegraph Circuits."

Another essential element of a "wireless" receiver was the detector, and Pierce's interest shifted to this aspect of the communications problem after General H. H. C. Dunwoody, USA (ret.), had announced in 1906 that the contact between a metal and polycrystalline silicon carbide (carborundum) would function as such a detector. Many other minerals were soon found to exhibit similar behavior, so Pierce designated them all as *crystal rectifiers* and "began a series of experiments to determine, if possible, the nature of the phenomenon." Interest in these devices fell off sharply a few years later as De Forest's three-electrode "oscillation detector" began to take over the role of detection as well as that of amplification in wireless re-

ceivers. As a result, the lucid phenomenological analysis of the behavior of crystal rectifiers that Pierce set forth in a series of five papers published between 1907 and 1912 remained almost unchallenged until the resurgence of interest in this class of semiconductors during and after the Second World War.

As an incidental by-product of these studies, Pierce made the first electroacoustical sound-level meter in 1908 by associating one of his molybdenite crystal rectifiers with a telephone receiver operated reversibly as a microphone. This instrument allowed him to make the first quantitative experimental study of the distribution of sound energy in a reverberant room, and to confirm thereby some of W. C. Sabine's conjectures and qualitative observations on this aspect of architectural acoustics.

As his studies of the general behavior of rectifiers progressed, Pierce was led to consider the possibility of exploiting the characteristics of electrical conduction in gases for the detection of electric waves. His experiments in this field soon guided him to the invention in 1913-1914 of a three-electrode mercury-vapor-discharge tube in which electrons were freed by ion bombardment at the surface of a pool of mercury at the bottom of the tube, whence they could fly through a grid toward a positive plate at the upper end of the tube. Under some conditions the potential of the grid would control the magnitude of this electron current, yielding amplification; under other conditions the grid would provide off-on control of the discharge current. In basic principle, therefore, this device behaved very much like the "thyatron" developed later by A. W. Hull of the General Electric Company, except that Pierce drew his electrons from a mercury pool, whereas Hull boiled them out of the surface of a hot cathode.

Pierce found that the characteristic mercury-vapor-discharge glow which concentrated near the anode in his three-electrode discharge tube would vary in brightness with the potential of the grid. In one of his five patents dealing with these tubes, Pierce showed how this effect could be exploited by using the signal from an antenna to

control the grid potential and by concentrating the light from the glow into a narrow line on a strip of moving film, thus creating a photographic record of the amplitude of the control signal. More than two decades later the Western Electric Company, by this time the holder of a license under Pierce's mercury-tube patents, was called on to defend itself in a Canadian court against an infringement suit addressed to their variable-density method of recording sound on motion-picture film. Western Electric's defenders resurrected Pierce's 1913 apparatus, which had been carefully preserved intact on a top shelf in his laboratory, connected it up with a modern microphone and amplifier, and proceeded to demonstrate to the court that a variable-density sound track could indeed be recorded on film as set forth in Pierce's patent specification. When news of this outcome reached Pierce, his reaction was characteristic. "I have just discovered that I invented the talking movies," he announced to his colleagues with evident satisfaction, and then added with a chuckle, ". . . more than twenty years ago."

While still largely preoccupied with his work on the mercury-vapor-discharge tube, Pierce collaborated with Professor Arthur E. Kennelly in 1912 on some measurements of the electrical impedance of telephone receivers. In a splendid manifestation of serendipity, these measurements led by happy accident to the discovery of the important concept of motional impedance. In making their measurements, one of the experimenters would balance the impedance bridge while the other tended the signal source, a not very reliable Vreeland oscillator located in a nearby room. Quite by chance one of them would habitually lay the receiver on its side on the laboratory bench while adjusting the bridge. The other always turned it face down, thereby altering the acoustic loading on the diaphragm, its motion, and hence the electrical impedance. It is easy to understand how alarmed they were when their measurements showed a complete lack of agreement at all frequencies in the neighborhood of resonance. In the course of pursuing the source of this discrepancy, they finally decided to abandon the careful nursing of the signal source

and *watch* each other balance the bridge, whereupon the difference in their procedures was at once apparent. Kennelly and Pierce both appreciated the physical significance of the effect immediately, and each independently worked out a substantially complete theoretical analysis of the phenomenon within a few hours.

In July, 1917, just after the United States entered the First World War, Pierce was granted leave to join the company of physicists assembling at the Naval Experimental Station in New London, Connecticut, for a study of ways and means to "do something" about the mounting losses due to enemy submarines. Such a gathering of physicists and engineers for common attack on a military problem can now be regarded as a precursor of the massive mobilization of civilian scientists behind the military effort of the Second World War.

The major effort at the New London "Station" was devoted to the perfection of devices for listening to the underwater sounds generated by a submarine's engines or its propellers. One group concentrated on passive all-acoustical systems; another, with which Pierce allied himself, concerned itself with electroacoustical systems using two or more underwater sound receivers, or hydrophones, attached to the ship or towed behind it. The advantages of such "multi-spot" systems were manifold: more sound energy could be intercepted, leading to greater sensitivity; and directional discrimination could be enhanced in much the same way that any listener's ability to locate the direction from which a sound is coming would be increased if his two ears were separated by several feet instead of by just the width of his head! In combining the signals from several hydrophones, however, it was necessary to introduce compensation for the differences in the time of arrival of the signals at each hydrophone location; and the devices for inserting the appropriate and controllable amount of delay in each signal path were called "compensators." Pierce made major contributions to this technology by inventing a new type of electric delay network (using mutual inductance) for which the time delay was essentially independent of

the frequency. What was equally important, he brought to bear on the practical problems of building these devices his background of experience, his ingenuity, and his flair for inciting others to constructive action. Always a master of the pithy remark and the adroit hyperbole, Pierce epitomized his concern with these practical problems by remarking years later, "We almost lost the war on account of dirty switch contacts and poor B-batteries."

After the war Pierce made his new constant-delay network the subject of a research paper and a patent application. In 1920-1921 he made further capital of his war-time research experience by beginning to offer for the first time in any university a graduate course in underwater sound signalling; and, for almost the entire interval between the two World Wars, the Navy would send to him each year a delegation of officers for postgraduate training in this field and in the other areas of electrical communication in which he had made Harvard an academic pioneer.

Pierce had begun to lay the foundation for leadership in this field almost as soon as he returned from his year of study abroad in 1901. In his first year of service as an instructor he introduced a new half-year course titled "Radiation." Two years later the catalog listed him for a "research" course in the field of "Radiation and Electromagnetic Waves," and the following year he introduced another pioneer course in "Electric Waves and their Application to Wireless Telegraphy." Six years later, in 1911-1912, he inaugurated still another new course in this field, designated as "Electric Oscillations and Electric Waves." To appreciate how far in the van this instruction was it is only necessary to recall that Marconi's classic experiment in trans-Atlantic "wireless" transmission was only ten years old in 1911-1912, and that the sinking of the *Republic* in 1909 and of the *Titanic* in 1912 had just furnished tragic proof that the safety of ships at sea required continuous "wireless" surveillance.

Pierce consolidated these academic beachheads by publishing two timely textbooks, each containing much that was new when it appeared. The first, *Principles of Wireless Telegraphy* (1910), was

based on the course he had been teaching for five years and summarized his research publications on tuned circuits and detectors. He gave the second, *Electric Oscillations and Electric Waves*, the same title as that of his later course, and its appearance in 1919 gave him the opportunity to make immediately available some of the results of his research on electric networks at the New London Experimental Station.

Pierce always regarded the position of Cruft Laboratory, midway between the Physics Department in the Jefferson Physical Laboratory and the School of Engineering in Pierce Hall, as more than a geographic accident. He maintained steadfastly that "Cruft" should be supported financially and sponsored academically by both the Department of Physics and the School of Engineering, and he guarded each of these bonds of affiliation with equal zeal. Of course, this tactical position inevitably supplied a basis for Pierce's dry comment: "The physicists call us engineers and the engineers call us physicists." Under his guidance, however, and with the stout support of Professor E. L. Chaffee, his colleague for thirty years, the term "Cruft Laboratory" came to signify not just a building but a whole scheme of applied science education based on vigorous liaison between pure physics and engineering practice. Chaffee's research interests complemented Pierce's, and their classes attracted students from near and far and gave training to two generations of leaders in the communications industry.

The 1920s can be called Pierce's "oscillator period." Two of his most important contributions were made during the middle half of this decade: the quartz-crystal "Pierce oscillator" in 1923 and the magnetostriction oscillator in 1927. Each produced one or more major publications and a portfolio of patents; each supplied the medium or the motivation for a family of doctoral theses; and together they shaped the remainder of Pierce's career.

The story of the "Pierce oscillator" is characteristic and revealing. The piezoelectric effect had been discovered as early as 1881, but it lay almost totally neglected until 1917, when Langevin began to ex-

ploit this property of quartz as the active principle of a transducer for underwater sounds. Professor W. G. Cady of Wesleyan University took up the study of these piezoelectric vibrators as soon as he heard of Langevin's work, and his experiments were chiefly responsible for opening up the field of frequency control by means of coupled electromechanical resonators. While working with a quartz crystal connected in the circuit of a self-excited vacuum-tube oscillator, Cady discovered in 1922 that the frequency of self-oscillation could be stabilized over a narrow range by the vibrations induced in the crystal.

Cady published his findings promptly, and demonstrated and described his crystal-stabilized oscillator circuits to many colleagues and friends, among them Professor Pierce. No argument was needed to convince Pierce of the technological importance of this development and he set about at once to repeat Cady's experiments. Within a few months he had invented three improved and greatly simplified forms of the crystal oscillator; circuits which differed from Cady's in that a *two*-terminal crystal would control uniquely the frequency of oscillation in a *single*-tube circuit. Each of the new circuits combined the virtues of simplicity and reliability: it would not oscillate at all unless the crystal was in the circuit, and then it would oscillate only at the resonance frequency of the crystal. As a consequence of these virtues, the "Pierce oscillator" came into almost universal use, and it has continued to dominate for more than thirty years the fields of frequency standardization and precision frequency control in radio transmitters and receivers.

Magnetostriction had been known and studied for more than forty years before piezoelectricity was discovered but, except for a handful of visionary proposals set forth in patent specifications, no practical use had been made of it before Pierce took up its study in 1926. Pierce took it as virtually a personal challenge when he found in a paper published that year the observation that "as far as can now be made out, no important application of the fact of magnetostriction . . . or of effects closely related to magnetostriction, has yet been

made," and with his background of experience with crystal oscillators it did not take him very long to get a self-excited magnetostriction oscillator going. Two coils surrounded a nickel rod: one in the output circuit to excite the rod's vibration, and one in the input circuit to sense the vibration and complete the feedback path. The correct polarity for the coil connections was discovered by making a mistake, Pierce said later. The connections that were wrong for exciting self-oscillation in an ordinary vacuum-tube oscillator turned out to be right when the back-coupling was furnished by the resonant vibration of the magnetostriction rod.

Magnetostriction vibrators, like quartz crystals, are "stiff" driving elements characterized by large forces but small displacements. This is just what is needed, however, to generate sounds in a relatively incompressible liquid medium such as water, and the emergence of magnetostriction in the interval between the two World Wars was timely. Pierce kept his longtime friend and fishing companion, Dr. H. C. Hayes of the Naval Research Laboratory, fully informed of his experiments, and magnetostriction was soon being exploited to energize sound projectors for underwater echo ranging. As a result, when the United States entered the Second World War, nearly all the antisubmarine vessels of the U. S. Navy were fitted with echo-ranging equipment using magnetostriction-driven sound projectors.

With typical ingenuity Pierce found many other new applications for magnetostriction. He used it to excite longitudinal vibrations in a nickel wire and to pick the signals off the wire after transmission over moderate distances, in a modern "carrier" version of the "lover's telegraph"; and he used the same principle to make a new kind of multi-receiver "compensator" that was functionally similar to those he had worked with in 1918. In another modification, using resonant rods for coupling instead of a wire, H. H. Hall developed with Pierce's guidance a linear-phase-shift narrow-band filter that was uniquely useful for the harmonic analysis of musical sounds of short duration.

While developing directional receivers for airborne or waterborne sounds, Pierce discovered that the transverse vibrations of a thick metal plate immersed in a fluid could produce sharply directional radiation in the fluid, and that even such a thick metal plate could be relatively transparent to high-frequency sound when the trace velocity of the incident sound wavefront matched the propagation speed of the transverse wave in the plate, a "coincidence" effect whose relevance to sound transmission through building partitions has only recently been appreciated. He combined magnetostrictive and non-magnetostrictive metals into bimetallic strips to make "bender" elements, and one of his doctoral candidates explored the use of such a vibrator as a phonograph pickup. He also contrived a novel form of large-diaphragm loudspeaker by attaching a magnetostrictive drive rod to the underside of a large lecture table. Surprisingly good reproduction was afforded by this arrangement, as Pierce demonstrated spectacularly on one occasion by climbing up on the table and remarking to a startled colloquium audience, "See, you can even stand on the diaphragm of this loudspeaker without interfering with its operation!"

The fine flair for showmanship exemplified by this incident was indirectly responsible for the trend of Pierce's activity during the last dozen years of his experimental career. He had once described what he called "the interesting moment" in the development of his crystal oscillator as follows: "Suspecting that the system was oscillating with frequencies above audibility a sheet of cardboard held in the hand was moved toward the crystal. A telephone in the circuit then gave out an audible note. This was due to the Doppler effect. The crystal was radiating sound of frequency too high to hear; the cardboard reflected the sound. On account of the motion of the cardboard the reflected sound had a different pitch from the emitted sound and hence produced a beat note . . . (which) on account of the rectifying effect of the vacuum tube was audible." This experiment not only demonstrated that the crystal was oscillating but was also a pioneer venture in making such high-frequency sounds audi-

ble, and it remained one of Pierce's favorite demonstrations. During the following fifteen years he made a series of progressively improved "supersonic" (now called ultrasonic) receivers for "translating" high-frequency sounds into the audible part of the spectrum; and with these receivers available to him, the ultrasonic sounds of nature had no defenses left against his curiosity.

It had been known since 1908 that flying bats would bump into obstacles if their ears were covered but not if the covering was shifted to their eyes. In 1937, D. R. Griffin, now Professor of Zoology in Harvard, appealed to Pierce for help in exploring whether bats could hear and emit sounds at frequencies above the human hearing range. They soon proved that bats did emit ultrasonic squeaks and chirps at frequencies as high as 40 to 50 kilocycles per second. That the bats also listened, in sonar fashion, to the echoes of their own signals and used these echoes for obstacle avoidance was also proved three years later by Griffin and Galambos in one of Pierce's basement laboratory rooms; and Griffin's continuing studies now show that these clever little flying mammals have anticipated many of our most sophisticated electronic techniques of echo-location.

A similar question had often been asked, but never answered, as to whether stridulating insects, such as grasshoppers and crickets, might not also emit superaudible sounds. Even without the challenge of this question it was perhaps inevitable that Pierce should have directed the sound-gathering horn of his frequency-translating receiver toward the crickets whose songs had lulled him to sleep at night during his summers in New Hampshire. He was soon able to show that these insects did emit high-frequency sounds, that the emissions were usually modulated at a relatively low frequency, and that the modulated ultrasonic bursts were sounded in typical rhythmic pulse-code patterns. He modified his apparatus to yield permanent records of these rhythms and devised an ingenious process of sound recording and rerecording with a speed differential which, in effect, slowed down the modulation enough to let each cycle be counted. In this way he proved that each modulation cycle corre-

sponds to the passage of one tooth of the insect's "file" over its "scraper," and his records established that the frequencies and the pulse-code pattern of each insect's chirps are sufficiently characteristic to qualify as a means of identifying the species acoustically.

These studies occupied "the large part of a happy decade," and supplied the material for his last book, *The Songs of Insects* (1948). "The casual reader may be interested," Pierce wrote in its Introduction, "to inquire how the efforts of a specialist in physics and communication engineering become directed to the study of insects. The answer might be that anyone who is ignorant has the obligation to seek enlightenment." There was, of course, a more specific reason in the fact that Pierce's background of research on the generation, propagation, and reception of high-frequency sounds qualified him uniquely for the pursuit of this enlightenment.

There was a wide disparity between the financial straits in which Pierce found himself when he arrived in Cambridge to begin graduate study and the secure financial position he subsequently achieved, largely with the proceeds from his patents. It is safe to say he did not care very much how large his fortune was so long as he had enough for comfortable living. He built an attractive home on Berkeley Place in Cambridge in 1914, and followed this in 1932 with a simple one for summer living in Franklin, New Hampshire, to which he annexed in 1936 a small laboratory to serve as a base for his insect studies. After his retirement in 1940, he built a large and comfortable winter home in St. Petersburg, Florida. He was always generous to the students who worked under him, many of whom were as needy as he had been at their age. This help usually took the form of employing them on very generous terms to assist him in his own research. In some cases these men were named as coauthors of the resulting papers; in others he encouraged them to publish independently, as Dr. Macfarlane had encouraged him to do at the beginning of his career.

An early episode revealed Pierce's pragmatic approach to financial problems. During negotiations for the purchase of his mercury-

vapor-discharge tube patents, the prospective buyer cautiously inquired what price Pierce had in mind. He replied without hesitation, naming an exact figure of so many dollars and so many cents. Since this price was substantially less than the buyer had been prepared to pay, an agreement was soon reached. When questioned later as to why he had named such an odd figure, Pierce replied that this was just the amount of the unpaid balance of the loan with which he had built his house in Cambridge, and that he was now out of debt! Later on, with a chuckle that always left his listener wondering whether he was serious or not, he would volunteer a formula which reduced the problem of investment to a similar basis of stark simplicity: "Put half of any surplus money you have into American Telephone and Telegraph Company stock," he would say, "and the other half into savings banks; then if the price of A. T. & T. goes down, you'll have money in the bank to buy more of it." His abiding faith in this corporation was based partly on his deep respect for the excellence of its research and partly on his personal connections with the men in it, many of whom had been trained in the Croft Laboratory. It can be doubted whether he followed his own investment dictum slavishly, but it did carry him safely and profitably through the financial crisis of 1929.

The sequence of events relating to the "patent phase" of Pierce's career was fortunate. The first thirteen of his fifty-three patents came before his "oscillator period" and all were salable. They yielded enough to assure him the comfortable living he sought and to give him a feeling of financial adequacy in undertaking the defense of his basic patents on the crystal and magnetostriction oscillators and on the other applications he found for piezoelectricity and magnetostriction. His early patents had not been seriously contested, but he soon found himself deeply involved in defending his "crystal" patents. For a lone college professor to lock horns in a patent interference contest with the American Telephone and Telegraph Company was a little like matching David against Goliath; but David won his match and so did Pierce, eventually, after twelve years of litigation.

The same A. T. & T.-owned patent that Pierce ran into had been cited earlier in interference with Professor Cady's application for a patent on his pioneer crystal-stabilized oscillator. Pierce always believed that Cady was entitled to his basic claims, and even after he won his own interference suit he refused to adopt any of the claims that had been derived from Cady's disclosures. He had urged Cady to defend these claims, even offering to underwrite the services of his own patent attorney, Mr. David Rines. Neither Cady nor Rines would accept this arrangement, however; and the latter generously insisted then, as he did later, on the privilege of helping impecunious academics protect their rights without regard for compensation. Cady soon tired of fighting it out even on this line and elected to sell his pending reissue applications for a modest sum that can be judged with hindsight to have been a fraction of their potential worth. On the other hand, although Pierce did not relish the litigation he encountered, he did not shrink from it when he felt that his just rights were being challenged. He never said it in so many words but the record clearly indicates that "it wasn't the money, it was the principle of the thing" that moved him. Except for the major contest mentioned above and one other "major" interference action, most of his litigation consisted of interferences relating to minor features of the crystal oscillator which Pierce felt obliged to defend for the protection of his licensees. He was victorious in most of these contests, but once his rights had been established he granted licenses liberally.

Due process followed a more equivocal course in connection with Pierce's magnetostriction patents. One of the principal users of magnetostriction in underwater-sound equipment manufactured for the U. S. Navy declined to take a license under Pierce's patents, and his suit for infringement was balked by the refusal of the Navy to reveal either in court or *in camera* the technical details of equipment still subject to military security classification. Pierce was deeply disappointed by this official reluctance of the Navy to acknowledge the use of his magnetostriction patents, and while he made his apparatus

and notebooks freely available to other scientists engaged in anti-submarine research during the Second World War, he chose to continue his own research on insects and to avoid further involvement in war research. Soon after the war ended, the legal impasse was broken by a substantial settlement out of court.

Outside of Harvard, Pierce was honored in various ways. He was elected to the American Academy of Arts and Sciences in 1907, and to the National Academy of Sciences in 1920. He served as the President of the Institute of Radio Engineers for two terms, in 1918 and 1919, and became the eleventh recipient of their Medal of Honor in 1929. The Franklin Institute awarded him the Franklin Medal in 1943 with the citation: "In recognition of his outstanding inventions, his theoretical and experimental contributions in the field of electrical communication, and his inspiring influence as a teacher." He was also made a fellow of various professional societies and, although he seldom traveled to out-of-town meetings after 1920, his office was a mecca for returning students and former associates, whose affectionate homage was the honor he cherished most.

Pierce had a certain shyness about his private affairs which made him keep the past to himself. In spite of his early liking for history, he said more than once that there was no interest in things of long ago; they were of no further use. In his active life it was the present and the immediate future that were exciting and full of promise. He was an excellent talker about scientific or academic matters, with a fund of good stories, many of which dated back to his Texas days. His facility in writing, and the breadth of his reading, helped him produce a small bound volume of commemorative and other poems and speeches which have a vivacity and wit that reminds one of Oliver Wendell Holmes. This collection begins with a few poems he had contributed to the *University of Texas Magazine*. He continued to add to it for more than fifty years, including specimens in Latin, French, and German, and he was always ready to decorate a festive occasion with timely verse.

Pierce was married in 1904 to Florence Goodwin of Saxonville,

Massachusetts, and they enjoyed forty-one happy years together. Although they had no children, they took great pleasure in the children of others, especially those of their neighbors, Cornelia and Walter Cannon (distinguished physiologist of the Harvard Medical School). The Cannons had a summer property in Franklin, New Hampshire, on part of which they invited the Pierces to build a summer cottage and later a small laboratory.

On one occasion, when the Pierces were taking off for a European tour in a sabbatical year, Mrs. Cannon warned Pierce that he might be bored and gave him a painting outfit. With no training but with much enthusiasm he proceeded to turn out portraits, landscapes, and more abstract subjects, one after the other, and for a while painting was his favorite hobby. The walls of his office were soon covered with portraits of friends, colorful katydids and grasshoppers, and symbolic pictures of electric waves leaping across oceans. His favorite, however, was a handsome rendition of Tenniel's version of Lewis Carroll's Dodo bird, with a superposed quotation that revealed its strong appeal to his instinct for experimentation. "'Why,' said the Dodo, 'the best way to explain it is to do it.'" He refused to acknowledge any difficulty or handicap in painting, but he made no claims, either, except for the evolution of another Pierce-like oversimplified formula. "Paint two pictures," he would say, "and then you can use first one and then the other as a guide in making improvements."

The death of Florence Pierce in 1945 left "G. W.," as he was affectionately known to colleagues and students alike, quite alone, with no children, no close relatives, and declining strength. He was fortunate in having good neighbors, however, who had long been friends of the Pierces. One of them was Helen Russell, of Sanborn-ton, New Hampshire, who proved to be a helpful and congenial companion. They were married in 1946, and Pierce continued to enjoy his annual pilgrimages between his home bases in Florida, Cambridge, and New Hampshire. He had experienced one minor "cerebral accident" in 1945, and a series of these recurred in 1956, from the last of which he did not recover.

The writers of this memoir have been forced to draw on many miscellaneous sources for information about Pierce's life, since what he himself left was inadequate. We have searched our own memories and those of his many colleagues and friends; and if there are more than the usual number of whimsical anecdotes scattered through this account, it is because they best reflect the gay spirit that is the most vivid recollection we all have of this colorful man. We are especially grateful in this connection to Mrs. Helen Pierce and Mrs. Walter Cannon. We have drawn freely on the material used in preparing the Minute on his Life and Services which was spread on the records of the Harvard faculty, and on the more detailed accounts published elsewhere¹ about the antisubmarine work at New London in 1917-18 and the patent litigation relating to the "Pierce oscillator." We are also indebted to Professor Robert B. Watson, now of the University of Texas, for pursuing Texas data for us; to Mr. David Rines for much useful information about Pierce's patent activities; and to the Franklin Institute for permission to use material from the biographical memoir submitted by the committee which prepared Pierce's nomination for the Franklin Medal award. His informal acceptance speech on the occasion of that award appears as the last entry in the appended tabulation of his publications.

¹ H. C. Hayes, "Detection of Submarines," *Proceedings of the American Philosophical Society*, 59(1920):1-47 and 371-404; and F. V. Hunt, *Electroacoustics* (Cambridge, Harvard University Press, and New York, John Wiley & Sons, Inc., 1954), pp. 53-57.

KEY TO ABBREVIATIONS

- Amer. J. Sci.=American Journal of Science
 Elec. World=Electrical World
 J. Acoust. Soc. Amer.=Journal of the Acoustical Society of America
 J. Franklin Inst.=Journal of the Franklin Institute
 J. Mam.=Journal of Mammalogy
 Phil. Mag.=Philosophical Magazine
 Phys. Rev.=Physical Review
 Phys. Zeits.=Physikalische Zeitschrift
 Proc. Amer. Acad. Arts Sci.=Proceedings of the American Academy of Arts and Sciences
 Proc. Inst. Radio Engrs.=Proceedings of the Institute of Radio Engineers

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UNITED STATES PATENTS

The disclosures published in these U.S. Patents were also covered by many foreign patents, none of which, however, anticipated the dates of the corresponding U.S. Patents.

TUNING APPARATUS AND CRYSTAL RECTIFIERS

Item	Patent No.	Issue Date	Filing Date	Title
1.	879,409	February 18, 1908	March 16, 1906	Wireless Telegraphy [Receiving system with "inductive connection," variable primary, variable secondary, and detector.]
2.	923,699	June 1, 1909	October 17, 1906	Wireless Telegraphy [Improved tuning apparatus.]

Item	Patent No.	Issue Date	Filing Date	Title
3.	879,061	February 11, 1908	January 11, 1907	Rectifier for Alternating Currents [“An electrical measuring instrument comprising a unilaterally conductive solid . . .” (carborundum).]
4.	879,062	February 11, 1908	April 5, 1907	Rectifier and Detector [Use of “conducting oxid of titanium” (anatase).]
5.	879,117	February 11, 1908	April 5, 1907	Rectifier and Detector [Use of silver-tellurium.]
6.	923,700	June 1, 1909	February 20, 1907	Rectifier [“In rectifying apparatus, . . . an asymmetrically conductive solid . . . to rectify and distort the alternating current. . .”]

(Items 1-6 were assigned at issuance to the Massachusetts Wireless Equipment Company but were assigned back to Pierce on April 10, 1913.)

MERCURY-VAPOR TUBE

7.	1,112,655	October 6, 1914	August 5, 1913	Apparatus for Receiving or Relaying Electric Signals
8.	1,112,549	October 6, 1914	November 26, 1913	Apparatus for Amplifying or Detecting Electrical Variations [Figure 2 shows arrangement for variable-density photographic recording of variable light from the glow at the anode.]
9.	1,087,180	February 17, 1914	November 26, 1913	Apparatus for Amplifying or Detecting Electrical Variations [Mercury-vapor tube with keep-alive circuit and a grid.]
10.	1,127,371	February 2, 1915	March 11, 1914	Apparatus for Amplifying or Detecting Electrical Variations [Improvement on Items 8 and 9 adapted for quicker recovery.]
11.	1,450,749	April 3, 1923	March 11, 1914	Apparatus for and Method of Controlling Electric Currents [Describes thyatron-like behavior. “From the nature of the gaseous conductor . . . it is much easier (by grid control) to start . . . than it is to stop the flow of current when once established.”]

(Items 7-11 were assigned on April 17, 1917, to Peter Cooper Hewitt.)

BIOGRAPHICAL MEMOIRS

COMPENSATORS AND DELAY LINES

Item	Patent No.	Issue Date	Filing Date	Title
12.	1,682,712	August 28, 1928	June 25, 1919	Electric Compensator [Developed at the Naval Experimental Station, New London. Made intensity maximum occur at same adjustment that yielded receiver compensation for binaural centering.]
13.	1,576,459	March 9, 1926	December 24, 1921	Electric Retardation Line [Delay line with mutual inductance between the series-connected coils. Filing date delayed by postwar negotiation for release.]

(Item 12 was assigned on April 24, 1922, and Item 13 on November 18, 1922, both to the Submarine Signal Company.)

CRYSTAL OSCILLATOR AND PIEZOELECTRIC APPLICATIONS

14.	2,133,642	October 18, 1938	February 25, 1924 Renewed April 18, 1930	Electrical System [The "parent" crystal-oscillator case. With the additions due to interferences and other causes, it ended up with 106 claims.]
15.	1,789,496	January 20, 1931	[February 25, 1924] January 18, 1928	Electrical System [A division of Item 14. Addressed generally to the production of constant-frequency beats in transmitter-and-receiver systems by "electromechanical vibrators."]
16.	2,133,643	October 18, 1938	March 29, 1926	Electrical System and Apparatus [Continuation-in-part of Item 14. Addressed to crystal holders and supports and other features. With additions from interferences and other causes, 120 claims.]
17.	2,112,863	April 5, 1938	January 5, 1928 Renewed February 27, 1935	Electrical System [A continuation-in-part of Item 14. A "Pierce Oscillator" with choice of two natural frequencies (different modes of vibration) by electrical switching.]
18.	2,133,645	October 18, 1938	January 14, 1928	Electrical System [Continuation-in-part of Item 14. Self-oscillating detector in radio receiver, with crystal control of self-oscillation or local oscillator frequency.]
19.	2,133,646	October 18, 1938	April 9, 1930	Electrical System [Continuation-in-part of Item 14. Oscillatory systems controlled by electromechanical vibrators; crystal frequency followed by frequency multipliers or harmonic selectors to yield stabilized high frequency in a transmitter.]

Item	Patent No.	Issue Date	Filing Date	Title
20.	2,133,648	October 18, 1938	July 10, 1931	Electrical System [Continuation-in-part of Item 14. Thickness vibrator; orientation of crystal "cuts"; variable impedance for adjustment of crystal frequency and other purposes.]
21.	2,266,070	December 16, 1941	[March 29, 1926] September 28, 1938	Electromechanical Vibrator Apparatus [A division of Item 16. More on crystal holders, including an evacuated housing or its equivalent.]
22.	2,133,647	October 18, 1938	June 25, 1931	Electromechanical Vibrator [Continuation-in-part of Items 14 and 16. Still more on crystal supports and holders.]
23.	2,133,644	October 18, 1938	January 9, 1928	Electrical System [More oscillator claims, extending Item 14; a receiving system.]
24.	1,962,155	June 12, 1934	April 23, 1928	Vibratory System and Apparatus [Receiving system for modulated carrier; crystal with microphone attached, conductivity of which varies in accordance with deformation.]

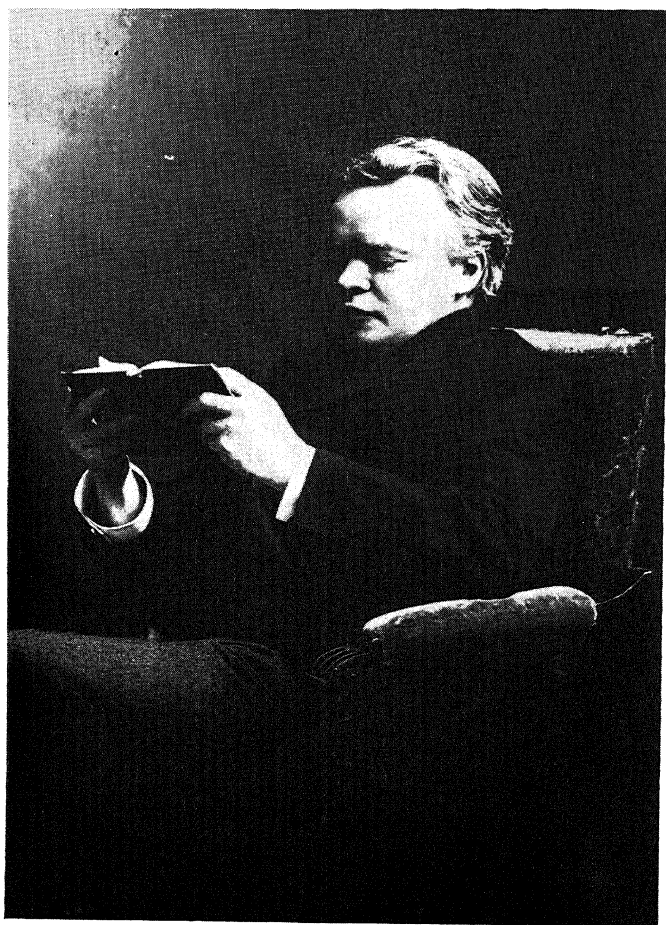
MAGNETOSTRICTION OSCILLATOR AND APPLICATIONS

25.	1,750,124 RE 19,461	March 11, 1930 February 12, 1935	January 3, 1927 June 26, 1931	Vibratory System and Method [The "parent" case for magnetostriction.]
26.	2,014,410	September 17, 1935	[January 3, 1927] July 20, 1928	Electromagnetostrictive Vibrator [A division of Item 25. Multiple-tube drive of diaphragm for radiating sound in water; segmented diaphragm.]
27.	2,014,412	September 17, 1935	[July 20, 1928] November 19, 1930	Magnetostrictive Transmitter [A division of Items 25 and 26. Small core-to-diaphragm area ratio; spatial distribution of the core means "to facilitate piston-wise cophasing of the parts of the diaphragm."]
28.	2,014,411	September 17, 1935	[November 19, 1930] August 2, 1933	Apparatus for Electromagnetostrictive Transmission and Reception [A division of Item 27 and continuation-in-part of Item 25. Multiply-driven diaphragm; reversible use as receiver.]
29.	1,882,396	October 11, 1932	March 23, 1928	Magnetostrictive Transformer [Coupling from one coil to another via magnetostrictive rod.]

Item	Patent No.	Issue Date	Filing Date	Title
30.	1,882,393	October 11, 1932	[March 23, 1928] August 9, 1932	Magnetostrictive Vibrator
	[A division of Item 29 and continuation-in-part of Item 25. Interchangeable rod vibrators; adjustable weights for frequency adjustment.]			
31.	1,882,394	October 11, 1932	[March 23, 1928] August 22, 1932	Magnetostrictive Vibrator
	[Another division of Item 29. Details of rod mounting.]			
32.	1,843,299	February 2, 1932	[June 28, 1927] April 23, 1928	Motor-Generator Set
	[A division of Appl. Serial No. 202,086, filed June 28, 1927, and later abandoned. Speed of motor controlled by using changing impedance of coil surrounding rod for frequency discrimination.]			
33.	1,962,154	June 12, 1934	April 23, 1928	Magnetostrictive Vibrator
	[Magnetostrictive vibrator with variable-resistance (carbon) microphone attached to generate signal for self-oscillation. Magnetostriction analog of Item 24.]			
34.	1,882,395	October 11, 1932	April 23, 1928	Frequency Indicator
	[Magnetostriction frequency-determining body.]			
35.	1,882,397	October 11, 1932	August 17, 1928	Magnetostrictive Vibrator
	[Composite vibrators, beaded rods, bimetallic strips, split tubes.]			
36.	1,882,398	October 11, 1932	[August 17, 1928] May 9, 1930	Magnetostrictive Vibrator
	[A division of Item 35. Composite vibrator made of concentric shells.]			
37.	1,882,399	October 11, 1932	[August 17, 1928] May 9, 1930	Magnetostrictive Vibrator
	[Another division of Item 35. Reed-type frequency meter with plurality of vibrators of different frequencies attached to common base.]			
38.	1,882,401	October 11, 1932	August 17, 1928	Loudspeaker
	[A continuation-in-part of Item 25. The "lecture-table" loudspeaker: Claim 4. "A loudspeaker comprising a magnetostrictive core weighted at one end and secured at its other end to a diaphragm."]			
39.	1,889,153	November 29, 1932	[August 17, 1928] February 26, 1931	Acoustic Electric Energy Converter
	[A division of Item 38. Magnetostrictive bimetallic strip driving a diaphragm.]			
40.	1,882,400	October 11, 1932	March 4, 1931	Vibratory Device
	[Magnetostrictive bimetallic strip used as phonograph pickup. S. A. Buckingham, coinventor, assignor to G. W. Pierce.]			

Item	Patent No.	Issue Date	Filing Date	Title
41.	2,063,950	December 15, 1936	December 4, 1931	Apparatus for Transmission and Reception
42.	2,063,951	December 15, 1936	[December 4, 1931] July 19, 1935	Apparatus for Transmission and Reception
	[A division of Item 41.]			
43.	2,063,952	December 15, 1936	[December 4, 1931] July 19, 1935	Apparatus for Transmission and Reception
	[Another division of Item 41.]			
(Items 41-43 were issued to Pierce's assistant, Raymond L. Steinberger, assignor to Pierce. They are addressed to the tube-and-plate type of transducer with diaphragm deeply slotted, both radially and annularly, to achieve in-phase vibration.)				
44.	2,063,944	December 15, 1936	February 9, 1932	Direction, Transmission, and Reception Method and System
	[Basic case on the "coincidence" effect.]			
45.	2,063,946	December 15, 1936	February 9, 1932	Sound Communication System
	[Sound transmission on a modulated ultrasonic carrier; transmission via longitudinal waves in a wire using modulated carrier; one form of compensator.]			
46.	2,063,949	December 15, 1936	February 11, 1932	Magnetostrictive Vibrator
	[Tubular vibrator with exciting winding inside the tube; coil housing of magnetostrictive material provides shielding and return flux path for solenoidal winding.]			
47.	1,997,599	April 16, 1935	July 19, 1932	Electric Filter
	[Improvement on Item 29. Coils carefully decoupled and shielded except for coupling through magnetostriction of vibratile core.]			
48.	2,031,789	February 25, 1936	August 9, 1932	Acoustic Electric Energy Converter
	[Loudspeaker diaphragm driver with one end of drive rod fixed; permanent magnet polarization.]			
49.	2,044,807	June 23, 1936	June 30, 1933	Transducer
	[Conical and other tapered couplers for transferring sound energy from medium to magnetostrictive wire. Issued to Pierce's assistant, Atherton Noyes, Jr., assignor to Pierce.]			
50.	2,063,948	December 15, 1936	June 30, 1933	Compensator and Method
	[Improvement over Item 52. Use of cone receivers, pantograph and linear guides to move pickup coils appropriate distances. Coinventor, Atherton Noyes, Jr., assignor to Pierce.]			

Item	Patent No.	Issue Date	Filing Date	Title
51.	2,063,945	December 15, 1936	August 2, 1933	Diaphragm and Method ["Coincidence" effect diaphragm made unidirectional by absorbing vibration at one end of plate.]
52.	2,063,947	December 15, 1936	August 8, 1933	Compensator [Multi-unit receivers, wire transmission, damping of reflected wave by elastomer wrapping distributed along end of wire.]
53.	2,014,413	September 17, 1935	May 11, 1935	Magnetostrictive Receiver [Continuation-in-part of Item 25. Frequency-discriminating sound receiver, plurality of tuned magnetostrictive bodies, tuned electrical circuits.]



Josiah Royce

JOSIAH ROYCE

November 20, 1855–September 14, 1916

BY ROBERT S. WOODWORTH

WHEN the University of California, in 1873, first opened its doors on the Berkeley campus, one of its entering students was a red-haired youth who had prepared for college in the San Francisco schools, after spending his early years in the gold-mining region. His parents had come across the Sierra in a covered wagon in 1849 and settled in the town of Grass Valley, where his father set up the "general store," and his mother continued her career as a school teacher by conducting a private school at home. Young Josiah's aspiration was to become a mining engineer, but he did not neglect the humanities—not by any means; his love of poetry and interest in religion converged into a graduation thesis on the theology of Aeschylus' *Prometheus Bound*.

That commencement address of 1875 proved to be a decisive turning point in Royce's career. Its outstanding excellence convinced some men in the audience that here was a young man who should have the opportunity of studying philosophy at the fountainhead! They accordingly raised a fund enabling him to spend a year or more at the German universities. On the way over he managed to stay a few days in Boston and make the acquaintance of William James, George Herbert Palmer, and others, on whom he made a very favorable impression. William James, especially, became his lifelong friend, and was instrumental a few years later in adding Royce to the brilliant and stimulating group of philosophers at Harvard.

Meanwhile, on returning from Germany, he secured a fellowship

in the just-opened Johns Hopkins University from which he obtained the Ph.D. in 1878. No adequate position in philosophy being available at the moment, Royce went back to Berkeley as instructor in English literature and logic. He held this position for four years, keeping up his philosophical activity by writing and by offering occasional short lecture courses. He also published his first book, *Primer of Logical Analysis for the Use of Composition Students*, designed to combat a certain "vagueness" which he observed in their writing. The subject of logic was indeed an abiding concern of Royce, especially in much later years.

An important event of this Berkeley period was his marriage in 1880 to Katharine Head, well remembered by generations of Harvard students as the gracious hostess of his pleasant home in Cambridge and the mother of two boys. She kept in close touch with his work and was able to collaborate in certain studies. The students used to credit her with a humorous description of his mode of lecturing: "This is the way Josiah lectures. First he tells you what he is going to say. Then he says it. Then he tells you what he has said; and finally he points out that he has said what he said he was going to say!" Royce's lecture style was in fact flowing and conversational, repetitious and abounding in pat examples, but never losing the thread or wandering from the point. He aimed to keep the class well oriented, knowing at all times whence they had come and whither they were going. This lecture style appears clearly in his published lectures on the *Spirit of Modern Philosophy* (1892).

In 1882, through the influence of James and Palmer, Royce was called from California to Harvard, at first on a temporary basis, but he was soon made a permanent professor and at Harvard he remained for the rest of his life. He did not become a "sessile organism," however, but journeyed hither and thither, giving lectures and lecture courses at many colleges and universities. The most notable of these lectureships were at Aberdeen in 1898-1900 and at Oxford in 1913. Several of his lecture courses are perpetuated in well-known books.

Royce's distinctive personality was well described, years after his death, by his Harvard colleague, George Herbert Palmer:

"He was a picturesque figure, a prodigious worker, a stimulating teacher, a heroic character, a playful and widely loved friend.

"His appearance was strange. His short stocky figure was surmounted by a gigantic round head well sunk in his shoulders. The top of it was sprinkled with red hair, while the strongly freckled face seemed to himself and to every stranger unparalleled in homeliness. . . . But no one who knew him well could wish a line of that face changed. Every inch of it expressed wisdom, modesty, humor. . . . His slowly sauntering gait was characteristic. And if you were short of time, it was not safe to ask him a question, however simple. . . .

"All knowledge was his province . . . psychology, logic, metaphysics, . . . mathematics, biology, . . . music and poetry, . . . the literature of England, Germany, France, and Italy, . . . common affairs of the day. . . ."¹

It was through religion that Royce was first attracted to philosophy. Bible stories read to him by his mother appealed to him strongly, while on the other hand rigid Sabbath observance was very repellent and the dogmatic theology he had to listen to aroused his "boyish fury." He felt that there were "problems" involved that could never be resolved by a dogmatic approach. His first major work was entitled *The Religious Aspect of Philosophy* (1885) and almost his last was *The Problem of Christianity* (1913). He was not concerned with specific denominational creeds; he wished to discover whether any basic, "absolute" knowledge was possible regarding reality and man's duty. He always insisted that knowing and doing were inseparable, so that what man could know and what he ought to do were integral parts of the same philosophic problem.

From his intensive study of Kant and the post-Kantian German idealists his own conclusion was a form of idealism. Reality must consist in experience—actual human experience continuous with in-

¹ In *Contemporary Idealism in America*, edited by Clifford Barrett (N.Y., Macmillan, 1932), pp. 3 ff.

finite realms of possible experience, our fragmentary experience continuous with a total, wholly organized experience. Any other philosophy he held to be radically self-contradictory, and he believed he could prove it to be so. Realism, for example, as an assertion of reality completely divorced from experience, independent of all experience and therefore unknowable, he held to be a self-destructive doctrine, a position which you could not defend without virtually abandoning it. "The realistic theory, . . . by its own explicit consequences, and just because its real objects are totally independent of its ideas, has nothing to do with any independently real object, and has no relation to the independent external world that its own account defines" (*The World and the Individual* [1900], I, 136).

The philosophical climate of the early twentieth century was by no means hospitable to Royce's idealism. Few of his many able students, devoted to him though they were, remained his adherents. His teaching fostered critical discussion and independent thinking, so that independence on the part of his pupils was to be expected. Yet, towards the end of his life, he "would sometimes express doubts about his own effectiveness as a teacher, because so many of the doctrines which he regarded as most distinctively original . . . seemed to him to have been still-born in the sense of having been received with barren respect, instead of being accepted, expounded, developed."² The realistic philosophy refused to die in spite of Royce's destructive criticism; a school of neo-realists emerged, some of them among his former students. Dependence, they argued, is a one-way relation so that our experience of a real object depends on the presence of the object, while nevertheless the existence of the real object is independent of our experience.

A glowing tribute to Professor Royce's personal interest in his students came from a leading member of the neo-realist group, W. P. Montague:

"I not only got from Royce my knowledge and appreciation of

² R. A. F. Hornlé, in Barrett's *Contemporary Idealism in America*, p. 300.

philosophy, but I got from him the kindest and most painstaking assistance in working out my own philosophic problems. He gave me this technical help through my five years of advanced study, and he accompanied it with continuous personal interest and affectionate counsel. . . . I owed everything to him, and it seemed mean and disloyal for me not to become his disciple. He was my dear teacher, and I longed to call him master, but I couldn't because his idealistic premises seemed to me false."³

The author of the present memoir, a contemporary of Montague, can add his word of testimony to the unselfish interest of Royce in his students. When the time arrived for me to make a choice between philosophy and psychology, I had a heart-to-heart talk with Royce and emerged from his study with a clear conviction in favor of psychology!

The intellectual spirit of those times, so strongly influenced by the theory of evolution, was especially hospitable to pragmatism, the view that human truth-seeking and investigation must be an outgrowth of the biological struggle for existence; any proposition would accordingly be "true," if, when put into practice, it contributed to the satisfaction of human needs. The major early proponents of pragmatism, Charles Peirce and William James, were much admired and respected by Royce. The issue of pragmatism versus absolute idealism was actively debated between Royce and James; and apparently James won Royce over to a certain extent. Indeed, Royce had always refused to separate knowing from willing; thinking was for him a form of purposive activity, even as the pragmatists asserted. But they could not dispense with the ideal of absolute truth as a goal to be approached by continued investigation, so Royce insisted. And how could they be sure that their practical success or failure was predictable from their hypotheses, unless they assumed the validity of logical deduction?

"In brief, Pragmatism presupposes a certain unity in the meaning

³ *The Ways of Things: A Philosophy of Knowledge, Nature, and Value* (N.Y., Prentice-Hall, 1940), pp. 653-54.

and coherence of experience taken as a whole—a unity which can never at any one moment be tested by any human being. Unless the propositions which assert the existence and describe the nature of this presupposed unity are themselves true, Pragmatism has no meaning. But, if they are true, Pragmatism presupposes a sort of truth whereof it gives no adequate account. To say this is not to say that Pragmatism gives a wholly false view of the nature of truth, but is only to insist upon its inadequacy. It needs to be supplemented.”⁴

“In so far as the validity of certain logical laws is concerned, we are obliged to maintain a position which we may characterize by the term Absolute Pragmatism. . . . There are *some* truths that are known to us *not* by virtue of the special successes which this or that hypothesis obtains in particular instances, but by virtue of the fact that there are . . . certain laws of the rational will, which we reinstate and verify, through the very act of attempting to presuppose . . . that these laws are not valid.”⁵

These “laws of the rational will” are, for example, the axioms of logic, geometry, or the theory of numbers—not regarded as self-evident but as postulates which, if maintained consistently in a logical or mathematical investigation, will generate a whole system of inter-related conclusions. Such a system, though purely theoretical in origin, is sometimes found to be of great value in applied logic or mathematics.

Royce’s interest in logic was of long standing; symbolic logic was a hobby of his. So, when Charles Peirce, himself a truly great logician, urged him (about 1900) to improve his philosophical argumentation by a serious study of modern logic, Royce was easily led in that direction, and his later contributions to logical theory were indeed substantial. In his *Principles of Logic* (from which an extract on pragmatism has just been quoted) he made a radical departure from the traditional sequence of topics. Instead of beginning

⁴ Royce, “Error and Truth,” in *Royce’s Logical Essays*, ed. by D. S. Robinson (1951), p. 118.

⁵ Royce, “Principles of Logic,” *ibid.*, pp. 364–65.

with the laws of thought as laid down in formal logic, and then proceeding to scientific methods, Royce began with an analysis of inductive methodology, and then proceeded to show how logical thinking depends on the general theory of order, of which the traditional formal logic furnishes only one instance. This treatment developed from the celebrated seminar in logic which Royce conducted for many years—a highly important and original course, as indicated in the obituary minute adopted by the Harvard Faculty of Arts and Sciences in 1916:

“His most notable contribution to the teaching of the University was made through his seminar in logic, which became a veritable clearing-house of science. Men of widely different training and technique—chemists, physiologists, statisticians, pathologists, mathematicians—who could not understand one another, were here interpreted to one another by Royce, who understood them all.”

This seminar left a powerful impression on those who were privileged to participate. As one of them has recently written, “It was said of Royce in this Seminar that he put out a challenge to anybody who had some idea to come in and fight. . . . Philosophical coöperation we got, . . . the clash of opposites under the guidance of a shrewd master who was the first among equals.”⁶

Royce found these joint discussions on scientific methodology of great value to himself in the development of his own ideas. One of his most interesting lectures (“Some Relations between Philosophy and Science in the First Half of the Nineteenth Century in Germany,” published in *Science* [1913] and partially reproduced in Robinson’s book, *Royce’s Logical Essays* [1951], pp. 260–67) was addressed to a group of Boston pathologists. For a couple of decades after 1800, as he informed his audience, the modern scientific movement had scarcely begun to take hold in the German universities. What was prevalent was the romantic school of idealism with its “philosophy of nature.” This *Naturphilosophie* practiced a method-

⁶ H. T. Costello, “Recollections of Royce’s Seminar on comparative methodology.” *Journal of Philosophy*, 53 (1956): 72–77.

ology which was almost the antithesis of scientific. It paid no heed to the principle of fair sampling, nor did it endeavor to formulate exact hypotheses from which deductions could be drawn and submitted to the test of experiment. Its aim was, by self-conscious insight and intuition, to organize the physical, chemical, and biological information then available into a rational idealistic system—as beautiful a system as could be conceived. The members of this school were engaged in an enthusiastic speculative enterprise. Nature, they felt, must have a basic unity pervading all its multiplicity. The human spirit demands that all truth shall hang together, and this was therefore a fundamental principle, a “leading idea” of the *Naturphilosophie*. Electricity, magnetism, light, the life processes must all be manifestations of a single force. The complex must evolve from the simple, and the way to understand the complex was to trace its development, its embryology.

The young German scientists of that era, especially in biology and medicine, were likely to be indoctrinated in this *Naturphilosophie* and to respond to the appeal of its “leading ideas.” As genuine scientific investigation began to take hold, however, this philosophic approach was seen to be a false lead. It was scornfully rejected by men like Johannes Müller and his pupils, Helmholtz the physiologist and Virchow the pathologist. Yet, as Royce took pains to show, its leading ideas remained as permanent guides in scientific advance.

In drawing the moral of his tale, Royce urged that, besides the inductive techniques of fair sampling and the testing of definite hypotheses, “. . . the progress of science largely depends upon still another factor, viz., upon the more or less provisional choice and use of what I have already called, in this paper, *leading ideas*.

“A leading idea is, of course, in any given natural science, an hypothesis. But it is an hypothesis which decidedly differs from those hypotheses that you directly test by the observations and experiments of the particular research wherein you are engaged. . . . It is usually of too general a nature to be tested by the means at the disposal of

your special investigation. Yet it does determine the direction of your labors, and may be highly momentous for you.

"Such a leading idea, for instance, is the ordinary hypothesis that even in the most confused or puzzling regions of the natural world law actually reigns, and awaits the coming of the discoverer. . . .

"The value of such leading ideas for a science lies in the sorts of research that they lead men to undertake, and also in the sorts of work that they discourage. . . . They awaken interest in vast ranges of observation and experiment, and sustain the patience and enthusiasm of workers through long and baffling investigations. They organize science, keep it in touch with the spirit of the age, keep alive in it the sense of the universal, and assure its service to humanity. Specialism, without leading ideas, remains but a sounding brass and a tinkling cymbal.

"The sources of useful leading ideas seem to me to be various. Social, and in particular industrial interests, suggest some of them. . . . The comparison of the results of various sciences awakens such leading ideas. . . . But another source . . . has been, upon occasion, philosophy. Philosophy itself might be defined as a systematic scrutiny of leading ideas."⁷

The titles of some of Royce's later books convey the impression of a shift of interest from logic and metaphysics to social psychology or sociology. Why should he speak so much of the "community," of "loyalty," and of "interpretation"? To such a question he replied that the community had fascinated him since his boyhood in the "young community" of the California gold rush and since his early history of that dramatic period. Gradually the mutual relevance of these two great interests of his had become clear to him. His studies of logic and of the mathematical theory of groups—strange as it may seem—established a bridge for him between the community and his metaphysics. A group of people achieves community of goal and loyalty by virtue of the internal "operation" of communication or, as he pre-

⁷ Royce, *Science*, n.s., 53:581, 583.

ferred to say, of "interpretation." A group of two persons is in a "dangerous" situation, for while loving each other they are likely to be mutually irritated and the hate will grow at the expense of the love. This "dyadic" situation can be greatly improved if transformed into a "triadic" relationship by the presence of a third person who adopts the role of a mediating interpreter. In larger groups, as well, the interpretative function is essential for harmonious and effective community action.

A well-functioning community can be regarded as a concrete infinite, like the series of whole numbers, or like the exponential growth curve. When you have defined the whole, your definition applies also to a part of that whole, since the series is self-repeating. A scientific community, consisting of the past, present, and future investigators in a certain science and their cumulative discoveries, is certainly a concrete reality; yet it is infinite in the sense that it has no determinable stopping place. It is self-repeating. And the role of the interpreter is obvious: past achievements must be made clear to the present generation; and each new discovery must be communicated to the scientific community, present and still to come. The role of the interpreter is especially obvious when scientific controversies arise.

In seeking for the broadest possible principle of community ethics, Royce reached the conclusion that "loyalty" was the best name for such a "leading idea"—loyalty to a cause, to the goal of the community. To be sure, the goals of different communities may be mutually conflicting and irreconcilable, as during a war. "Loyalty to loyalty" is still good, but the ideal must be the Great Community which can be beloved of the whole human race. A businesslike approach to such a universal community, Royce suggested in his 1914 book, *War and Insurance*, might be made by the establishment of an international insurance system, which could insure individual nations against the calamities of earthquakes, pestilences, hurricanes, famines, and finally against the war risks of any nation which abstained from committing the first act of a war. "In so far as our in-

surance company undertook to pay any war expenses, it would get a businesslike interest in averting the causes of war . . . until we get such a community of interpretation formed as to teach the nations, by the potent devices of mutual insurance, the art of loyalty to the community of mankind."

During his lifetime, Josiah Royce received many honorary degrees and other marks of high esteem. He was elected to the American Academy of Arts and Sciences in 1891, to the National Academy of Sciences in 1906, and to the American Philosophical Society in 1908.

He was president of the American Psychological Association in 1901, and of the newly formed American Philosophical Association in 1903.

In spite of the apparent eclipse of Royce's philosophy shortly after his death in 1916, recent years have brought much renewed recognition of his outstanding contributions, as shown by the following books and articles: Herbert W. Schneider, *A History of American Philosophy* (N. Y., 1946); John E. Smith, *Royce's Social Infinite* (N. Y., 1950); D. S. Robinson, ed., *Royce's Logical Essays* (Dubuque, Iowa, 1951); James Harry Cotton, *Royce on the Human Self* (Cambridge, Mass., 1954); "In Memoriam Josiah Royce" (articles by ten American philosophers), *Journal of Philosophy*, 53(1956):57-139.

KEY TO ABBREVIATIONS

- Calif. Univ. Bull.=California University Bulletin
 Educ. Rec.=Educational Record
 Internat. Jour. Ethics=International Journal of Ethics
 Internat. Mo.=International Monthly
 Jour. Philos.=Journal of Philosophy
 Jour. Spec. Philos.=Journal of Speculative Philosophy
 Philos. Rev.=Philosophical Review
 Proc. Amer. Philos. Soc.=Proceedings of the American Philosophical Society
 Proc. Amer. Soc. Psych. Res.=Proceedings of the American Society for Psychological Research
 Psychol. Rev.=Psychological Review
 Trans. Amer. Math. Soc.=Transactions of the American Mathematical Society

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Lyndon J Small

LYNDON FREDERICK SMALL

August 16, 1897–June 15, 1957

BY ERICH MOSETTIG

LYNDON FREDERICK SMALL was born in Allston, Massachusetts, on August 16, 1897, the second child (and the second son) of Frederick Eugene Small and Amanda Edmond (Corey) Small. His grandfather, Ebenezer Small, Jr., was a physician, and Small's mother also had strong inclinations toward a medical profession. On various occasions, Lyndon Small has indicated to us his own early desire to become a surgeon.

In 1905 Small's father moved with his wife and three children (Harold Corey, Lyndon Frederick, and Dorothy) to Needham, Massachusetts.

From his earliest boyhood, through his years of high school and college, and his adult life, Small was devoted to swimming, skiing, hunting and fishing, camping, and mountain climbing. From the little one could extract from a man not much inclined to social talk, particularly when his own history was concerned, one picture came out quite clearly: His years as a child and a boy were happy ones. A genuine respect and friendship bound him to his father, his brother Harold, and even more to his sister, Dorothy. From his father, a machinist and inventor by profession and a strong personality and individualist by nature, Small acquired not only many personality traits, but also a love for the lathe and the machine shop. The lathe he inherited from his father followed him for many years, even to Charlottesville and to Bethesda. Wherever the Smalls lived, in their city residence or at their beach home in North Carolina, the

machine shop was an important part of the home and, particularly in his later years, the mainstay of his recreation. There is no doubt that intimately connected with Small's love to produce with his hands were not only his unusual skill in glass-blowing, equal to that of a professional, and his perfect mastery of and love for Pregl's microanalysis, but above all his astounding skill as an experimentalist—astounding because of the simplicity of technical means with which he conducted the most complex procedures. A few test tubes and glass rods, a Bunsen burner, and a simple lens were the most important items of his technical arsenal.

The first person who had an influence in shaping Lyndon Small's future was Frank P. Bunker (Dartmouth '02), science teacher of the Needham High School, who recognized Small's penchant and talent for physical sciences and nourished and furthered them to the utmost. Under the stimulus of this man, young Small began to realize that there was a lot to be gained by continuing his formal education. "This is a debt I can never repay, except in the scientific results." He enrolled in Dartmouth (1916) on a budget that would be ridiculous today, but he managed to make out. Because of financial pressure perhaps, but more likely because of love of knowledge for its own perfection, in his first semester he stood first in his class of some 400, an achievement which made him eligible for the unusual freshman fellowship in the second semester (this fellowship supported him throughout the Dartmouth years). This academic recognition, moreover, had a corollary in the form of complete discretion granted such students over attendance at classes, an advantage which was promptly used for time in the mountains and woods. This paradise of education and exploration of the White Mountains was rudely interrupted in October of 1917, the beginning of the sophomore year, by a polite invitation to join the U. S. Armed Forces in the First World War. Small joined, with little or no reluctance, the U. S. Coast Artillery at Fort Standish, Boston Harbor, where he was "taught to give enemas to constipated mules, to sleep on corn husks (pretty good) and to fend off tarts on Atlantic Avenue." He also had to

learn that it was obviously poor tactics to receive a diagram of a machine gun in the morning and know more about it by noon than the sergeant did. After six months of conflicting rumors that his outfit was to be sent abroad, having never fired a rifle or any of the many impressive guns that decorated the fort, those who had positions waiting for them were released. Though "College Student" was not classified as a position, he managed to get out. The release from the Army allowed him now to return to college work—as far as it could be conducted in the mountains. He became Phi Beta Kappa in 1919 and received his first degree from Dartmouth, that of B.S., in 1920.

After graduation Small took a summer job at Camp Serrana, in Pike, New Hampshire, as "hiking and camping counselor." On the train up to his summer's destination he happened to meet a young girl by the name of Marianne C. Brown, also going to the camp as "the secretary to the camp director." The following summer the hiking counselor and the director's secretary were engaged to be married. From 1920 to 1922, Small was a Henry Elijah Parker Fellow from Dartmouth College at Harvard University where he received his M.A. degree (under the late Professor Kohler) in 1922.

In June, 1922, Lyndon and Marianne were married. The honeymoon was spent in true Small fashion, the groom and bride roaming the White Mountains for three months, each loaded with 40- to 50-pound packs, including tents, axes, fishing and cooking equipment, food, and blankets.

To keep the home fires burning, Small took the position of instructor in inorganic chemistry at the Massachusetts Institute of Technology for the academic year 1922-23, and the following summer both Smalls functioned as swimming instructors at Camp Aloha in Fairlee, Vermont. The following three years (1923-26) Small spent at Harvard (as assistant, instructor, and Du Pont Fellow) working under James B. Conant for his Ph.D. degree, which he received in the summer of 1926. Professor Conant, to whom Small was strongly attached, influenced Small's career considerably. The sum-

mers of this period of arduous work and professional development were spent in summer camps, both young parents (their first child, Donald Clayton, was born in April, 1924) teaching young girls in Camp Alleghany, West Virginia, swimming and diving. The baby came along free as the camp's mascot. Upon the recommendation of Professor Conant, Small was awarded the Sheldon Travelling Fellowship (for one year) and the Small household moved to Munich in September, 1926. The funds for a second Munich year were provided by a National Research Council Fellowship.

These two years in Munich were decisive ones for Small's future. There was, first of all, a close relationship between the great master Heinrich Wieland and the young, eager disciple who was bent to absorb the maximum from the wisdom, knowledge, and experimental skill of his new teacher. Small adapted himself very fast to his new surroundings and became in no time even more Bavarian than the Bavarian students, donning *Lederhosen* and *Lodenrock*, heavy stockings, and all that goes with them for his daily street-wear, whereupon Wieland called him affectionately his "Amerika Seppl." Small's frank and refreshing ease of manners, his sincere respect for greatness and achievement free from stiff and bureaucratic formality, quickly won Wieland's heart, and at once gained him the friendship of his new European colleagues. Heinrich Wieland and Lyndon Small died less than two months apart. Responding to a letter of condolence by Mrs. Small, Mrs. Wieland wrote: ". . . und danke Ihnen sehr, auch im Namen meiner Kinder. Sie haben ja meinen lieben Mann auch so gut gekannt, und wissen freilich was wir verloren haben . . . Auch ich traure mit Ihnen, liebe Frau Small um Ihren lieben Mann, den wir alle hier so gerne hatten; er ist ja viel zu früh von hinnen gegangen. . . ."

The Smalls were taken graciously and with open arms into the Wieland household and spent many happy hours there, becoming particularly attached to the Wielands' children, Eva (now Mrs. Feodor Lynen) and Theodor (now Professor of Chemistry at the University of Frankfurt). Lyndon was given laboratory space in the

"Kalbstall" which he shared with Maximilian Ehrenstein (now at the University of Pennsylvania Medical School), Wilhelm Wenner (now with Hoffmann-La Roche at Nutley, New Jersey) and Fräulein Dane (still at Munich). The laboratory brought him into close contact with Clemens Schöpf, and a lifelong friendship developed between them. Of three research projects that Professor Wieland suggested to him, Small chose the alkaloid problem, "The Ozonization of Thebaine." Within two years he acquainted himself thoroughly with the complex chemistry of the morphine alkaloids which became his life's work, as can be seen by a mere glance at the list of his publications. With very few exceptions, his publications from 1928 to 1956 deal with structural features and reactions of morphine, codeine, and thebaine.

The Smalls made good use of the time allotted to them in Europe. There were numerous visits to academic and industrial laboratories, a microanalytical course in Pregl's laboratory, and an extensive visit to the Glaswerke of Jena. For recreation there were many hiking trips, long *Faltbootfahrten* on the Rhine, the Inn, and the Danube, the climbing of Mount Etna and, to crown everything, the ascent to the top of the Matterhorn, no small alpinistic achievement at that time. The Smalls left Munich with heavy hearts in 1928.

Professor Conant had asked Small to come back to Harvard as his private assistant, an offer which Dr. Small gladly accepted. It developed, however, that the Department of Chemistry of the University of Virginia was looking for a research associate, and offered this position, upon Conant's recommendation, to Small. He accepted and, after his return from Europe, moved directly to Charlottesville, continuing immediately his research with morphine alkaloids. At the same time Small built up a microanalytical laboratory and gave courses in microanalysis for graduate students.

In 1929 the Division of Medical Sciences of the National Research Council established a committee under the chairmanship of William Charles White, with the purpose of finding and studying means to reduce or correct the evils of drug addiction. The theoretical con-

ception, based in part on the replaceability of cocaine by the safer novocaine, was the separation of the addiction liability of morphine from its other salutary properties by chemical modification. Such a program necessitated the organization of chemical, pharmacological, and clinical studies. It was a fortunate coincidence that a man uniquely competent to direct the chemical studies was available, and Dr. Small, again through the recommendation of Dr. Conant was appointed Director in Charge of Chemical Research, continuing at the same time in his position of research associate at the University of Virginia. The University was most generous in providing space for the "Drug Addiction Laboratory."

The new assignment gave Small the opportunity to build up a research group of considerable size, consisting of Ph.D.s and graduate students, and it was during these ten years that Small performed a long series of masterly and painstaking researches on morphine and allied compounds. In addition he wrote, assisted by Professor R. E. Lutz of the University of Virginia, *The Chemistry of the Opium Alkaloids*, which for the first time marshalled together the whole chemistry of the alkaloids contained in opium. Although today somewhat out-of-date and, unfortunately, out of print, this book is still the most comprehensive and informative monograph on the subject and is found on the desk of everyone working with morphine alkaloids.

Dr. Small approached the problem of finding non-habituating drugs in two ways. First, it could be expected that by proper modification of the functional groups of the morphine molecule compounds might be found in which undesirable effects were diminished and analgesic effect perhaps enhanced. The second approach was to introduce functional groups of morphine into simpler ring systems and stepwise build up synthetics resembling in one or more ways the morphine molecule. Small concerned himself primarily with the first approach. Although it seemed that morphine chemistry had been fairly well exhausted and that not much could be discovered or added to the earlier extensive researches, particularly by

German and British scientists, Dr. Small unearthed with ingenuity and tenacity a wealth of new reactions and new transformation and degradation products. There was discovered and structurally elucidated a long series of desoxycodines and desoxymorphines, their respective codeinones and morphinones, and their alkylation and reduction products. Notable among these efforts was the synthesis of metopon, which represented the first known nuclear-alkylated morphine derivative. This substance proved to be an excellent analgesic and showed promise of being superior to morphine in that its use was attended by a less rapid development of tolerance and dependence. Further, it could be administered orally and produced fewer side effects than morphine. Because of these attributes its general use in the control of chronic pain was advocated.

Particularly intriguing to Small was the chemistry of thebaine, its complex reduction products and, finally, the most difficult of all, the chemistry of the phenyl dihydrothebaines. Here, Dr. Small and his collaborators produced the most unexpected and fruitful results. Again and again throughout all these studies Small's unusual experimental skill, the simplicity of methods applied, and his immense amount of patience became evident. The hundreds of morphine derivatives so produced and pharmacologically studied by Dr. Nathan B. Eddy, at the University of Michigan, established well-defined correlations between structure and activity. This was done by analyzing all the effects produced by individual changes on the molecule, such as muzzling of the phenolic hydroxyl, covering, substituting and eliminating the alcoholic hydroxyl group, changing steric factors, substituting the nucleus, and altering the nature of the nitrogen function. Many of the new morphine derivatives were tested in man for addiction liability and some (metopon, desomorphine, monoacetylmorphine) were proved to be superior by further clinical testing.

With the rather rapid and continuous appearance of some fifty papers by Dr. Small and his associates, Small's reputation as an alkaloid chemist and specialist in morphine chemistry grew accord-

ingly and he was elected to a number of important offices. He was Chairman of the Division of Organic Chemistry of the American Chemical Society in 1936; Consultant to the U. S. Public Health Service from 1929 to 1939; U. S. Technical Representative to the League of Nations in Geneva in 1931, and a member of the Opium Assay Commission in London in 1937.

In 1939 he received (jointly with Dr. Nathan B. Eddy) the first Annual Scientific Award of the American Pharmaceutical Manufacturers Association. In 1938 Dr. Small became editor-in-chief of the then two-year-old *Journal of Organic Chemistry*. In this office, where he was so ably assisted by his wife, he earned the lasting gratitude of American organic chemists. His thoroughness, keen and clear judgment, tact and impartiality, his unsparing effort and personal editing of every paper, brought this publication to the standard of a first-rate professional journal. As a token of appreciation his friends and colleagues dedicated the papers in the November, 1957, issue to his sixtieth birthday anniversary, which he was not to see.

Everyone associated with Small during the Charlottesville years (1928-39) will remember this period with joy and pride. It did not take the Smalls long to become an integral part of the University of Virginia community. The beautiful natural setting of Thomas Jefferson's university, the gracious hospitality and culture of Southern society added a happy tone to the serious and hard work carried on in the "Drug Addiction Laboratory," where almost regularly the lights burned deep into the night. To continue work after dinner until all hours was an accepted rule. The life in the laboratory was that of a large family. Mrs. Small (called the "Frau Vorstand" in reminiscence of the Munich days) was officially the laboratory's secretary, but essentially its executive officer. Since she had acquired by that time an astounding knowledge of chemistry, her assistance in putting the numerous papers into publishable shape was invaluable. As the writer well remembers, all of us depended on her to an almost unpardonable degree, handing to her not more than rough drafts and having returned to us the final reorganized, corrected, edited,

and neatly typed papers. Greatly adding to the spirit and atmosphere of Small's laboratory was the genuine and intense interest shown by the Chairman of the Drug Addiction Committee, Dr. White, in the progress of the research at Charlottesville and of the pharmacological unit at the University of Michigan. The intimate friendship with Dr. White greatly shaped Small's thinking and acting. The passing away of this outstanding man in 1947 was a keenly felt shock to Small and to all of us who had the honor to work under Dr. White's leadership.

The Smalls, their family having been enlarged by daughter Ruth (now Mrs. Edward D. Farren of Kensington, Md.), acquired a large and comfortable house with a good-sized yard which induced Small to add to his hobbies a new one—that of gardening. This was pursued with as much thoroughness and skill as his other hobbies, and the multitude, beauty, and variety of his flowers attracted wide attention. Their home was open to anyone in the laboratory and to all their friends at any time. Once a year there was a big "laboratory party" to be remembered by everyone who attended. The same hospitality extended to their beach home in North Carolina, where many of us spent happy days of fishing and swimming which are still remembered.

When in 1938 the outbreak of the Second World War was a near certainty, Dr. R. E. Dyer, then Director of the National Institute of Health, inquired whether Dr. Small and some of his key men would be willing to shift, at least temporarily, their interest from morphine substitutes to quinine substitutes. The decision for Dr. Small probably was not an easy one. But eventually he and a small group of his associates, among them the writer, moved to Washington, D. C., and were attached as a unit to Professor Claude S. Hudson, Director of the Division of Chemistry of the National Institute of Health. In this new field Small and his associates moved ahead energetically and successfully, and became part of the wide-flung antimalarial program of the Office of Scientific Research and Development. Hundreds of compounds were synthesized, among them a dozen or more

derived from polynuclear hydrocarbons, effective enough to replace quinine and atabrine. Many were studied intensively, pharmacologically and clinically.

In 1941 Small was elected to the National Academy of Sciences, and he received in 1949 the Hillebrand Prize of the Washington Section of the American Chemical Society for outstanding research on alkaloid chemistry.

After the war Dr. Small was eager to return to basic research. Although his resignation from the editorship of the *Journal of Organic Chemistry* in 1951 made more time available for his own work at the bench, so dear to him, new duties restricted considerably his research activities. He was elected Member of the reorganized Committee on Drug Addiction and Narcotics, National Research Council, in 1947; Consultant to the Technical Command, Army Chemical Center, Maryland, in 1950; Chairman of the Post Office Advisory Committee in 1951; and Member of the United Nations Opium Committee in 1954. In 1951 he succeeded Professor Hudson as Chief of the Laboratory of Chemistry, National Institute of Arthritis and Metabolic Diseases.

Upon the outbreak of the Korean war in 1950, Dr. Small was authorized to reassemble what remained of the old "Drug Addiction Laboratory" personnel, whose immediate objective was to develop adequate, total-synthetic morphine and codeine replacements, because of the obvious threat to our opium source. From the efforts of this group emerged several clinically efficacious synthetic analgesics of greater potency and less side-action liability than are inherent in morphine and codeine, which could serve as capable substitutes for these two naturally occurring drugs should the need arise. At the same time significant progress was made along theoretical lines. Dr. Small's most recent and personal research efforts were directed toward the elucidation of the structure of certain unusual thebaine derivatives, and the results will be published posthumously.

Dr. Small's excellent command of the English language made him an outstanding lecturer, and it was a great pleasure, unfortunately a

too rare one, to listen to his clear and precise presentations. Although Small appeared retiring in his professional daily life and even somewhat aloof, he was a close and true friend to all his associates. He was a man with a courageous and kind heart.

On June 9, 1957, he was awarded *in absentia* the honorary degree of Doctor of Science by Dartmouth College, where he had begun his career as a chemist forty-one years before. He died in the early morning hours of June 15.

KEY TO ABBREVIATIONS

Ann.=Justus Liebig's Annalen

Ann. N. Y. Acad. Sci.=Annals of the New York Academy of Sciences

Ann. Rev Biochem.=Annual Review of Biochemistry

J. Am. Chem. Soc.=Journal of the American Chemical Society

J. Org. Chem.=Journal of Organic Chemistry

J. Pharmacol. Exp. Therap.=Journal of Pharmacology & Experimental Therapeutics

P. H. R. Supp.=Public Health Reports Supplement

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LEWIS MADISON TERMAN

January 15, 1877–December 21, 1956

BY EDWIN G. BORING

LEWIS MADISON TERMAN, for fifty years one of America's staunchest supporters of mental testing as a scientific psychological technique, and for forty years the psychologist who more than any other was responsible for making the IQ (the intelligence quotient) a household word, was born on a farm in Johnson County, Indiana, on January 15, 1877, and died at Stanford University on December 21, 1956, a distinguished professor emeritus, not quite eighty years old.¹

When a biographer seeks to find causes for the events in the life that he is describing, he is apt to find himself facing the nature-nurture dilemma, uncertain whether, in order to account for the traits of his subject, he should look to ancestry or to environment. Terman, as it happens—when he wrote his own biography at the age of fifty-five (1932)—faced exactly this problem in accounting for himself.² In his choices he must indeed have been influenced by the *Zeitgeist*, for, as the weight of scientific opinion shifted from hereditarianism toward environmentalism, his judgment shifted too throughout the forty years (1916–1956) during which this issue remained vital to him.

¹ The best account of Terman's life up to 1931 is autobiographical: L. M. Terman, *A History of Psychology in Autobiography* (1932), II, 297–331. Briefer appraisals of his total contribution are: E. R. Hilgard, "Lewis Madison Terman: 1877–1956," *Amer. J. Psychol.*, 70(1957):472–79; R. R. Sears, "L. M. Terman, Pioneer in Mental Measurement," *Science*, n.s., 125(1957):978f.

² Terman, "Autobiography," pp. 297–305 *et passim*.



Lewis M. Terman

In 1916 Terman published what came to be called the Stanford Revision of the Binet test of intelligence, a test that measured intellectual growth in youth from three years of age to adult intelligence, which seemed to be reached at sixteen years or perhaps a little sooner.³ The measure of intelligence that was supposed to remain invariant with age is the intelligence quotient, the ratio of mental age (average intellectual test-performance for a given age) to chronological age (times 100). That idea had been William Stern's in 1912,⁴ but Terman, by improving the tests for mental age, was able to demonstrate that the IQ is indeed fairly constant with changing age, at least when cultural influences are also constant. If the IQ is constant from an early age, then adult intelligence is predictable from childhood, and it becomes easy to suppose that the intelligence measured is an invariant fixed at birth and quite possibly at conception by the genes. Terman was supported in this view by the growing realization at this time that feeble-mindedness cannot be greatly altered by training, and by the definition of *feeble-minded* in terms of the invariant IQ. The IQ was, of course, not strictly constant, but its variability and its regression toward mediocrity with advancing age could be laid to imperfections in the tests, perhaps to their lack of validity in measuring the fixed underlying intelligence in which so many had come to believe.

In the teens and twenties liberal opinion fought this view of a biologically elite intelligentsia, focusing attention on such changes in the IQ with age, education, and socio-economic status as were discovered. Terman, nevertheless, stuck to his original view. His initiation in 1921 of his genetic studies of genius, studies that were still being continued with the examination of the same group of gifted persons at the time of his death thirty-five years later, was based on his belief, which he got from Francis Galton, that the brains of the country are one of its great resources and that they can be selected with scientific

³ Terman, *The Measurement of Intelligence*, 1916.

⁴ William Stern, "Die psychologischen Methoden der Intelligenzprüfungen," *Ber. V Kongr. exper. Psychol.*, 1912, pp. 1-109, esp. on the IQ, pp. 25-29.

procedures and used to advance the national welfare and civilization. Thus in 1932, when he ventured to lay down eighteen articles of faith in a credo that he printed at the end of his autobiography, his eighth belief was "that the major differences between children of low and high IQ, and major differences in intelligence test scores of certain races, as Negroes and whites, will never be fully accounted for on the environmental hypothesis."⁵

As time went on this faith of Terman's in a basic invariant intelligence for every person weakened a little. The evidence that test scores for intelligence depend on socio-economic status increased. By the time of the Second World War the use of factor analysis to establish separate primary abilities, especially L. L. Thurstone's work,⁶ had cast doubt on the unitary nature of intelligence. The I, as well as the IQ, was getting into trouble. Terman in his personal copy of his autobiography wrote in the margin opposite the sentence just quoted about Negroes and whites, "I am less sure of this now (1951)!" and later, "And still less sure in 1955!"⁷ On the other hand, his continuing study of the gifted children, at last grown up after twenty-five years, reinforced his belief—in spite of a certain small regression of the top group toward mediocrity as age advanced and the realization that achievement depends on motivation as well as on intelligence—that there is in society under its ordinarily constant conditions an intellectual elite who need to be identified and specially trained and encouraged for the promotion of civilization.

It is hard to say whether Terman's faith in the importance of heredity made him, when he came in 1932 to assess the causes of his own success, discount the effects of environment, or whether, unable to see anything in his circumstances as a poor farm boy that could have led him into an intellectual life of national importance, he looked to heredity for an explanation because, being inscrutable, it could not disappoint him. Certainly he then seemed at pains to show

⁵ Terman, "Autobiography," pp. 329f.

⁶ L. L. Thurstone, *Primary Mental Abilities*, 1938.

⁷ Hilgard, "Lewis Madison Terman," p. 478.

that there was nothing either in the commonplace of his rural youth nor in the agricultural lives of his immediate forebears that could explain the intellectual avidity that made him ultimately successful in the scientific world. Nor would his belief that heredity must be of great importance be weakened when his own son, F. E. Terman, was also elected to the National Academy of Sciences.⁸ Lewis Terman must have believed that intelligence was there in his ancestors, undisplayed because the environment failed to yield or permit the necessary motivation.

Whatever their origin, this biographer sees two important characteristics that, appearing early in the life of Lewis Terman, supported him to the end. In the first place, he had tremendous drive and persistence that lasted in his pursuit of knowledge from the time he entered the Central Normal College in Danville, Indiana, at the age of fifteen, all through his life, in spite of setbacks with tuberculosis, being burned in a fire, breaking a hip, and other disabilities. Always he returned indefatigable and enthusiastic to his work. His views of it might change or the facts of nature might force a change; still he followed obstinately the same track, not with a dour stubbornness, but in a friendly, sympathetic, social activity in his contacts with colleagues, students, and his "gifted children." There is something special there that is part of his success.

The other characteristic that marked his life was his inextinguishable desire for reading, which began when he was about ten years old, at which time his brother bought a book on phrenology. (The man who was selling the book had felt Lewis's bumps and predicted great things for him.) Lewis read the *Britannica* in his father's library and most of the other tenscore books that his father owned. At the Central Normal College, when he was sixteen and seventeen, he was reading John Dewey's *Psychology*, Darwin's *Origin*, Huxley's *Lectures*, and other books of that sort. He read William James's *Principles of Psychology*, quite new then, surreptitiously because his in-

⁸ Lewis M. Terman was elected to the National Academy of Sciences in 1928; his son, Frederick E. Terman, was elected in 1946.

structor disapproved of the book's literary flavor. Later, when he got to Indiana University, came under the influence of W. L. Bryan, E. H. Lindley, and J. A. Bergström, and decided to become a psychologist, his reading of the "right" things continued. While at Indiana he also mastered French and German so that, when he went to Clark University later, he could read adequately for Stanley Hall's seminar in three languages. This addiction to the use of books persisted until the end. Lewis Terman was a well-read man and not only within his special field.

Enthusiasm and tenacity, plus wide and well-chosen reading, seem to have been important factors in Terman's career. Nevertheless the autobiographer, as well as the biographer, faces an inevitable dilemma when he looks for causes of how his subject came to achieve eminence. If the biographer can find an environmental explanation he is apt to accept it *faute de mieux*. Often the acceptance of the alternative hereditarian explanation is due merely to the failure to find an environmental one, nor is the case proved by marshaling a few bright ancestors when all the other forebears have disappeared in the obscurity of the past.

With Terman it is possible to make a case for his inordinate will to achieve as a compensation for a frustrated youth, but that does not account adequately for his love of learning. Had he read only what would immediately promote his success, we could suggest that early frustration could be at work here too, but the fact is that he read more broadly, seemingly just for the joy of it, than his professional advancement ever required. Terman himself inclined toward an hereditarian explanation of himself, noting proudly his son's eminence. What he failed to note in this connection was the fact that his two older brothers did not rise so high, that his married sisters did not manage, in choosing their mates, to mark themselves off as exceptionally gifted, in the way that the bright girls of Terman's own "gifted group" did when they married. You have to take Terman as a fact. Considered alone he is not a good instance of environmental effect or of heredity.

Now let us see what are the facts that belong on this central core of the intellectual endeavor that is the key to an understanding of Terman's life.

YOUTH AND EDUCATION

Lewis M. Terman was the twelfth child among the fourteen children of James William Terman and Martha P. Cutsinger. James Terman was a farmer, the son of a Virginia farmer of Scotch-Irish descent who was born in 1794, fought in the war of 1812, migrated on horseback to Ohio about 1820, moved on to Indiana in 1846, taking James with him, and died there in his seventies shortly after the Civil War. This Terman, Lewis's grandfather, had married a woman named Jones of Welsh extraction. They had twelve children, of whom James was one of the youngest.

Martha Cutsinger, Lewis's mother, was the daughter of a Pennsylvania-German farmer who went from Pennsylvania to Kentucky and thence to Indiana. He had married a woman named Deupree of Huguenot origin. As a young man James Terman went to work for Cutsinger, married his daughter in 1855, and then moved away to a farm of his own in another part of the same county. Lewis, as we have noted, was born in 1877, after his parents had been married twenty-two years.

In this all-agricultural environment it would have seemed a safe prediction in 1890, say, when Lewis was thirteen, that he would become a farmer, marry young, have a large family who were destined to become farmers and farmers' wives except for a couple who would be school teachers, an alternative rural possibility. Lewis worked on his father's farm for five or six months every year from the time he was eleven until he was eighteen. He fitted easily into the work and did not dislike it, yet his avidity for knowledge was beginning to show and presently made the difference between a farmer and an intellectual. From the age of five to the age of thirteen he attended a one-room rural school of about thirty children and one teacher. He stayed on for a year after he had finished the eighth grade and ap-

parently spent the next two years on the farm. Perhaps this was the crucial moment, or at least what Terman himself would have regarded as the crucial moment, when he was led to decide that he wanted something that the farm would not provide, something that he could get at the Central Normal College at Danville.

Terman must later have come to believe that his success in the intellectual world of science meant that he himself was a "gifted child" in the phrase that he later coined in his genetic study of genius, that he had superior intelligence that could nevertheless have remained submerged under the routine of a farmer, where industry is repetitious and originality is confined to those small inventions that never emerge to alter even slightly the course of civilization. No wonder he believed so firmly that genius needs to be discovered, directed, and reinforced.

In 1892, when Lewis was fifteen, his parents sent him to the Central Normal College. He stayed two years, for thirty weeks the first year and twenty weeks the second. Then he taught a one-room rural school for a year and went back to the Central Normal College the next year for forty-eight weeks to complete the "scientific course" and receive the degree of B.S. After that he taught a rural school again and went back to the College, this time for eighteen weeks to complete the course in pedagogy and receive the B.Pd. degree. Still his eagerness for more and more education was not satisfied, so now, without stopping to earn more money, he borrowed enough to spend forty-eight additional weeks at Danville, thus completing the "classical course" to receive an A.B. Altogether he had spent one hundred sixty-four weeks at the Normal College, had three degrees to show for his work, yet remained vague as to what the future held for him.

Terman was now twenty-one years old. For the next three years he was principal of a township high school where he taught all the courses to about forty pupils. After the first year he married Anna B. Minton, a teacher whom he had met at Danville. Their son Frederick was born the next year. Both Lewis and Anna were sure that Lewis needed more schooling.

By this time Terman had decided that he wished to prepare himself to teach pedagogy and psychology, and he looked to Indiana University where W. L. Bryan, the Stanley Hall-trained psychologist from Clark University, was teaching. Indiana University was only fifty miles from his home. His friends advised him to go there to get a better A.B. than the Normal College could confer. To do so he had to borrow money again, though living in Bloomington was relatively cheap.

At the University he found, besides Bryan, E. H. Lindley and J. A. Bergström, both of them, like Bryan, Clark Ph.D.s. After Terman's first year Bryan became President of the University and was lost to Terman. Bergström was not so easy to know, but Terman came after a while to appreciate him and also to realize that experimental psychology, Bergström's *Fach*, was not for him. Terman was awkward with apparatus and never could learn to adapt himself to the mechanical phases of psychology's then new experimentalism. Especially Terman liked Lindley, who presently was steering him in the direction of Stanley Hall and Clark.

At Indiana, Terman improved his French and German so that he could read the literature in psychology without great difficulty. He became quite excited over the new scientific movement in German psychology, even though he was not prepared himself to become an experimentalist. He read a great many more books, received a solid Indiana A.B. at the end of his first year, an A.M. at the end of his second, and borrowed \$1,200 more to go on to Clark University and a Ph.D. with Stanley Hall. Hall was still one of the giants of the new American psychology, a contemporary of James's, almost twenty years older than Cattell and Baldwin, the founder of the first American psychological laboratory and the second in the world. No wonder Terman's ambition soared at the thought of a Ph.D. with Hall.

It was in the spring of 1903 that the Termans had their second child, a daughter, Helen (now Helen Terman Mosher and living in Stanford). Little Fred was then almost three. Yet neither Terman nor his wife demurred about taking this family of four to Clark on

a borrowed \$1,200 for a Ph.D. which turned out to be two years away. Terman owed his success not only to his own courage and ambition, but also to his wife's courage and her identification with his ambition.

The two years at Clark, when Terman was twenty-six to twenty-eight years old (1903-1905), were wonderful years for the farm boy so avid for education and now at last within sight of a Ph.D. He wrote of the University:

"The Clark of my day was a university different in important respects from any other that ever existed in America—. . . in spirit much akin to the German university yet differing from it because of the small student body. It enrolled in all its departments only about fifty full-time students. . . . Possibly thirty of the fifty were there primarily for psychology, philosophy, and education. The informality and freedom from administrative red tape were unequalled. The student registered by merely giving his name and address to President Hall's secretary. He was not required to select formally a major or minor subject. There was no appraisal of credentials for the purpose of deciding what courses he should take. *Lernfreiheit* was utterly unrestricted. There were professors who proposed to lecture and there were students who proposed to study; what more was necessary? The student could go to three or four lectures a day, or to none. No professor, so far as I could see, kept a class list. Attendance records were, of course, unheard of. No marks or grades of any kind were awarded at the end of the year or semester. One could attend a course of lectures all year without being required or necessarily expected to do the least reading in connection with it. There were no formalities about candidacy for a degree. The student was allowed to take his doctor's examination when the professor in charge of his thesis thought he was ready for it. No examination except the four-hour doctor's oral was ever given."⁹

Yet *Lernfreiheit* was not quite so easy as it sounds. Terman de-

⁹ Terman, "Autobiography," p. 313.

scribed the intense motivation generated by the demand for a report in Hall's famous Monday evening seminar. The reporting student who did not weather the storm of criticism from his peers, reinforced as it might be by the deliberate and recondite negatives of the seminar's famous moderator, might go home for a week in bed, and one in Terman's day had a nervous breakdown when he realized his failure.

At Clark, Terman had contact with E. C. Sanford, the experimentalist, but it did not make him apparatus-apt. He sat under W. H. Burnham and heard his polished lectures on education and educational psychology, and he learned to perceive the beauty of an English style that Hall did not have. (Perhaps his own later facility as a clear and interesting writer owes something to Burnham.) From every quarter he felt the influence of European psychology as it went on in Germany with the new laboratories, in France with Binet and the measurement of intelligence, in England with Galton and the mental tests. Hall was, of course, his chief stimulus, though Hall did not support Terman in the two subjects of his special interest: the mental tests and the study of superiorly intelligent children. Hall was, however, never coercive, and Terman chose for his thesis the comparison of seven dull with seven bright children by the use of a great number of tests of Terman's own devising. Earlier Terman had presented to Hall's seminar a survey of the history of belief about bright children, how they had come to be looked down upon and regarded as instable and abnormal, likely to be a weight upon society instead of an aid to it. This view Terman intuitively opposed, and he set himself to test it by devising tests for bright children. Thus his principal life endeavor began at Clark in 1904. He took his Ph.D. under Hall in 1905, when he was twenty-eight years old.

MATURITY

Terman's life is so associated with difficulties of health and accident that inevitably a biographer pauses to wonder whether the

effort to overcome the obstacles may not indeed have encompassed other activities and have been one of the causes of his success.¹⁰ At any rate the road to achievement, after he had obtained the Ph.D. at Clark, was no easier than the road he had already traversed.

The chief enemy was tuberculosis. There had been hints of it in 1899 and 1900, but the first serious hemorrhages occurred in the summer of 1904, in between the two years at Clark. He rested a few weeks and then took special care of himself during his second and last Clark year. He knew he needed to find a position in the south or southwest, but that requirement did not daunt him, for Stanley Hall was so closely identified with what was then the modernization of education under the impact of the new psychology that many of his Ph.D.s took administrative or teaching positions in normal schools or high schools. That kind of position could be found where the climate would be favorable, as might not have been the case for a university post.

Within three days Terman had opportunities to go to Florida, Texas, and San Bernardino, California. He chose the last, going there as a high school principal. He had another hemorrhage a few weeks after the term began, but he rested for eleven days and was soon back at work. What he regarded as a successful year ended with an offer of a professorship of child study and pedagogy at the Los Angeles State Normal School, an offer which he accepted.

He was at Los Angeles for four years (1906-1910). The library was good. The work was interesting. There were other psychologists there who stimulated him—Arnold Gesell and Beatrice Chandler, who later married each other. There grew up a friendship with

¹⁰ Quite early experimental psychologists discovered that a supposed distractor of the attention does not always distract but may act as a motivator so that the effort to resist distraction actually spurs the attention, and achievement is improved by that which was expected to interfere with it. See, for example, A. J. Hamlin, "Attention and Distraction," *Amer. J. Psychol.*, 8:3-66, and M. A. Tinker, "A Study of the Relation of Distracted Motor Performance to Performance in an Intelligence Test," *Amer. J. Psychol.*, 33:578-83. Alfred Adler's conception of compensation for sensed inferiority is a similar principle.

E. B. Huey, an old Clark man. In the summer of 1907 the Termans, the Gesells, and Huey were together on vacation. Later, when Huey was working in Adolf Meyer's clinic at Johns Hopkins, and Terman was about to move to Stanford, Huey urged Terman to undertake work with the new 1908 Binet scale for measuring intelligence—and Terman did, publishing the Stanford revision six years later.

Bergström, by whom Terman had been taught at Indiana, was called to Stanford in 1909 as a psychologist in the Department of Education, but he died before the year was over. Stanford then asked Huey, who decided, however, to stay with Adolf Meyer a little longer. After that Stanford turned to Terman, a third Stanley Hall man, and he accepted.

Terman went to Stanford in 1910 as an assistant professor of education in E. P. Cubberley's department, and was promoted to be an associate professor in 1912 and a full professor in 1916. These first twelve years (1910-1922), before he was given the Department of Psychology to build up, were for him still maturational. He was changing from a psychotropic educationalist—the Stanley Hall model—to a sociotropic psychologist. He published three books on health and school hygiene—"If you scratch a health reformer, you will find an invalid," he used to say—but his enduring achievement was the revision of the Binet scale for measuring intelligence.

Up to 1916 Terman's identification had been largely with educators and educational psychologists. He had felt himself to be on, or just beyond, the periphery of the American movement in psychology, as so many of Stanley Hall's men were—the American movement that had its core in experimentation, the new laboratories, and basic fact, the "brass-instrument" psychology, as James called it. Terman at Stanford, disliking the brass instruments which existed across the way in Frank Angell's laboratory, was intellectually and geographically remote from this core of psychology. He was a "mental tester," and the experimentalists looked down on his art, some because it had little to do with consciousness and some because it was applied science. All that was shortly to change.

It was in 1916, the year of the publication of the Stanford Revision of the Binet scale, that Terman went back east to teach in the summer session at New York University. The next summer he taught at Columbia. At that time he actually was not a member of the American Psychological Association, so peripheral did he feel, but he joined in 1917. It was in April, 1917, that the United States declared war on Germany and that the psychologists mobilized themselves to see if they could render aid, especially by testing recruits for intelligence. R. M. Yerkes, because he was then President of the American Psychological Association and also because he was peculiarly well suited to the task, took charge, and late in May a committee of five distinguished "mental testers" met at Vineland, New Jersey, to plan for the psychologists' war effort. Terman, who was one of these five, stayed with the work and finally, in uniform and commissioned as a major, was responsible for the 200,000 words of Part II of the mammoth report that was published in 1921, *Psychological Examining in the United States Army*.¹¹ Hardly had he become a member of the American Psychological Association than he was elected to its Council (1919-1921) and then to its presidency (1923). The psychologists liked Terman and they came—even the conservatives who had contemned mental testing—to respect his ability. Thus the shift of Terman from the periphery of professional American psychology to its core was very rapid in these half dozen years.

In 1922 President Wilbur of Stanford asked Terman to become Executive Head of the Department of Psychology, because of the retirement of Frank Angell, one of Wilhelm Wundt's students, who had been in charge of Stanford's laboratory for thirty years. Terman accepted and his title was changed to Professor of Psychology and Education. This was the sort of challenge to which he would rise with all his drive and energy. The old department had granted only one Ph.D. in the thirty years of its existence. Terman's job was to

¹¹ *Psychological Examining in the United States Army*, ed. by R. M. Yerkes, *Mem. Nat. Acad. Sci.* (1921), vol. 15. Terman was responsible for Part II, "Methods of Examining: History, Development, and Preliminary Results," pp. 293-546.

build it up, and build it up he did. By 1949 three members of his staff, besides himself, had become presidents of the American Psychological Association and members of the National Academy of Sciences: W. R. Miles, C. P. Stone, and E. R. Hilgard. Of the Stanford students who were there during Terman's incumbency, four more became presidents of the American Psychological Association and two members of the Academy (H. F. Harlow and Heinrich Klüver). From the point of view of Academy representation in psychology, Stanford in the Terman period ranks among the five top institutions. From 1922 to 1942, when Terman retired, the Department of Psychology conferred 55 Ph.D.s. Certainly the scientific contribution of Stanford in psychology passed in this period from little significance to great. The growth could not, indeed, be all Terman's doing. Such developments, once started, are autocatalytic. One good appointment favors others, but Terman's tolerant appreciation of scholarship in any field of psychology, his constant effort to get the best men and then to treat them with a permissive democracy, established a philosophy that made Stanford a very good place to be. He got the process of change going and then steered it until he retired.

Terman took great pride and personal interest in his students and in other students too, for whom he acquired a preceptorial relation, and also in his younger associates on the staff, many of whom had been appointed as the result of his efforts. A list of these students and associates includes many names of great importance in present-day psychology. A score of them we can list here. In 1942, on the occasion of Terman's sixty-fifth birthday, a group of these persons issued a volume, called *Studies in Personality*, to commemorate their debt to Terman for his stimulus and sponsorship. Contributing to the volume from among his graduate students, who wrote their theses under his direction, were Barbara S. Burks, Florence L. Goodenough, Catherine Cox Miles, R. R. Willoughby, and Kimball Young. Contributing from among his other students and protégés, all persons who felt a very real and almost filial debt to him, were

R. G. Barker, Franklin Fearing, H. F. Harlow, L. P. Herrington, E. Lowell Kelly, John L. Kennedy, Heinrich Klüver, Ann Magaret (now Garner), F. L. Ruch, R. R. Sears, Eugene Shen, Miles A. Tinker, and Clare Wright (now Thomson). Signing the salutatory preface were, from among his graduate students, R. G. Bernreuter, Quinn McNemar, and Maud A. Merrill, besides Miss Goodenough, Mrs. Miles, and Kimball Young. To these names one ought at the very least to add John W. Gardner, Donald G. Marquis, and Neal E. Miller. An able and distinguished group this, all of whom had felt Lewis Terman's stimulus and stood ready to do him honor.

INTELLIGENCE AND THE GENETIC STUDIES OF GENIUS

Terman's lifework, the persistent core of his scientific contribution, was the study of the nature of exceptionally high ability. He first acquired this interest, so he claimed, in his work with Lindley at Indiana University in 1902. It was, as we have seen, the subject of his thesis in 1905 at Clark University with Stanley Hall. His chief concern was at first with bright children, and in this attack he was driven by his conviction of the falsity of the common belief that brightness in children is undesirable, that the very bright children are apt to be sickly or weak or neurotic or maladjusted. Later, as his research continued to support his conviction, he denounced the conventional thesis and speculated, as had Francis Galton before him, on how to increase the number of gifted children in the community, how to discover them and to make their ability available to society.

When Terman came to Stanford in 1910 he had the opportunity to begin serious work on this topic. Since the Binet-Simon scale of intelligence had appeared in 1908, he now had at hand for the selection of bright children a means that had not been available when he was at Clark. In 1911 he selected from certain schools a group of 31 children with IQs in excess of 125, described their abilities and characteristics then, and undertook to see what happened to them later. By 1924 one of these children had a Ph.D., another an Sc.D., and a third was studying in Europe.

Terman's own revision of the intelligence scale in 1916 gave him a still better tool for the selection and for charting the development of the tested children as they grew into adulthood. It was becoming plain that it was important not only to show that bright children are not queer and maladjusted but also that they grow up, with IQ fairly constant, into valuable, competent, bright adults. It was in this way that the study of gifted children changed into the *Genetic Studies of Genius*, as Terman called the big volumes, filled with facts, that came out during the last thirty years of his life.

In 1921, just before he became executive head of Stanford's Department of Psychology, Terman secured his initial support from the Commonwealth Fund for the selection and study of a thousand gifted children from the schools of California. Terman and his associates could not test the entire school population of California—about 160,000 pupils—but they had the teachers select the three brightest children in each class, according to the teachers' judgment, and they also took the youngest child in each class. This last criterion turned out to be the best. These selected children were then tested and those with IQs of 140 and over were retained (and a few in the 130s). Thus the study obtained 661 bright children, the top half of the highest one percent of the school population. Later the investigators added 365 more to obtain "the thousand." Still later others were added for special reasons, making the total group of gifted children 1,528.

Information was obtained about these children's home life and their school life. They were given medical examinations; anthropological measurements were made on them; school-achievement tests and character tests were given them; their interests, the books they had read, and the games they knew were inventoried. Here was a picture of the whole child, as well as it could be obtained. The description was published in *Genetic Studies of Genius, I, Mental and Physical Traits of a Thousand Gifted Children*, in 1925, with Terman as author and fourteen assistants, some of them later to be distinguished psychologists, listed on the title page.

The second volume of the *Studies* was, in a sense, a control for the first. It was called *The Early Mental Traits of Three Hundred Geniuses*, appeared in 1926 (a year after the first volume), was authored by Catherine M. Cox (now Miles) with Terman as one of her assistants, and consisted of 842 pages of analysis of the youth and young manhood of three hundred persons who had attained great distinction because of ability. The biographical facts were studied and an IQ estimated for childhood and another for youth. A measure of reliability was computed, dependent upon the nature and amount of data available for appraisal. These posthumous IQs for history's great were naturally of much comparative interest—Goethe with an estimated IQ of 210, Descartes at 180, Darwin at 165, and the child who became Napoleon at 145, just barely gifted enough for Terman to add to his group. The real purpose of the control was, however, to compare the childhoods of history's geniuses, with especial reference to their IQ performances, with the IQ performances of California's child "geniuses," that is to say, of the children with IQs of 140 or more. There was every evidence that history's great were bright as children and that their intellectual performance in childhood fell within the range of Terman's gifted children. The highest IQs, for both groups, were in the neighborhood of 200.

The third and fourth volumes of the *Studies* were follow-ups. The first resurvey occurred in 1927–28, six years after the initial one. It was published in 1930 under the title *The Promise of Youth* and under the joint authorship of Barbara S. Burks, Dortha W. Jensen, and Terman. Then there were resurveys in 1936, 1940, and 1945. The results of these came out in the fourth volume of the series. *The Gifted Child Grows Up*, by Terman and Melita H. Oden, published in 1947, about twenty-five years after the first study and when the gifted group averaged about thirty-five years of age. Terman retired to become emeritus in 1942, but he kept on at work without interruption. He was in constant correspondence with his gifted "children," as he still liked to call them, and a fifth volume with Mrs. Oden was under way when he died in 1956. She will continue the

work. It will show the "gifted group" thirty-five years after the genetic studies began, at about age forty-five. *The Gifted Child at Mid-Life* will round out the enterprise.

The general conclusion as of 1947 was that the gifted group continued to have superior physique and health. Their achievement quotients remained through school as high as their IQs. They tended to be versatile, not specialized. In school these children had nearly always been placed in classes above their age but below their capacity. They were not more subject to personal maladjustment than normal children, and that statement holds for the very high IQs above 170 as well as for those between 140 and 170. Vocational achievement rates were high among the group. Marital happiness was normal or a little above normal. Aptitude for marital success was good. There was some little regression of ability toward mediocrity, but only such as should be expected on statistical grounds. The fertility rate at that time was not sufficient to maintain the stock. Many marks of eminence were discovered among the men, and some among the career-minded women.

It is nevertheless clear that a high IQ, though maintained into adulthood, is not a sufficient cause for eminence. Sir Francis Galton believed that genius would emerge even against unfavorable conditions. The truth seems to be that it may not emerge as eminence, even under otherwise favorable conditions, when motivation is lacking. Intelligence alone is not enough. Ambition, career-mindedness, drive may also be necessary to obtain from men and women of ability the maximal contribution to civilization.

Terman suggested that capacity for contentment is also great among gifted persons and that this kind of success must not be overlooked, especially when one is examining the lives of able women who marry. Here lies what must at first have been an unforeseen development in these genetic studies of genius. It is not enough to be able to discover genius by measuring ability and selecting the top persons. Given the material for achievement, it has nevertheless to be energized. There can be, moreover, two kinds of success: society's

success that comes through the discontent that drives men of great ability to great achievement and the eminence that marks it, and the individual's success which gives him—or her—the contentment which would make him wish to live the same life over again if he had the choice but does not drive him to notable public accomplishment.

It is clear that these studies represent the taxonomy of genius. Terman's forte was description. It was not a simple naturalist's description. He used statistics to reveal hidden attributes. He was persistently ingenious in thinking up ways to measure new dimensions of ability, inventing tests and scales to support the big descriptive task. These books are crammed full of carefully related facts, systematized and put in order. There is, however, in them very little theory that is more than description, little desire to gain simplicity by the creation of conceptual entities. The exception is *intelligence*. To that reified construct Terman held vigorously. It was something that could be measured. The different tests might not wholly agree, but it was the tests that were at fault, he thought, as they got differently at the basic thing. Intelligence as a potentiality seemed pretty well fixed in childhood, and probably it was for the most part inherited. That is the way it seemed.

When factor analysis tended to split up "intelligence" into a number of primary abilities, Terman resisted the new thinking. He had too great an investment in intelligence to let it go readily. In the Second World War, however, intelligence tests by that name were abandoned, and the Army General Classification Test was used to measure three of Thurstone's seven primary abilities: verbal ability, numerical ability, and the comprehension of spatial relations. When this biographer remarked in 1955 that the concept of intelligence was on the way out, Terman protested; yet that was also the year of his marginal note about his being less sure than ever that intelligence (at least as the tests test it for Negroes and whites) is inherited.

Along with the scientific study of gifted children goes Terman's great practical achievement, the Stanford Achievement Test, a test

for mastery of school subject matter at every school grade from the second to the tenth. The original work was done in collaboration with T. L. Kelley and G. M. Ruch and was put on the market in 1923. As school curricula changed the test was revised in 1929, 1940, and 1953. It has four different forms, which show a very high correlation one with another. The standardization is now based on the performance of 345,736 school children drawn from 363 school systems in 38 states. It is the best-known of Terman's tests. Millions of school children have taken them. It would seem that Terman's feeling of financial insecurity disappeared as this test succeeded in the early 1920s, and it is not without interest to note that his drive for achievement did not diminish, so far as a biographer can tell, with the arrival of economic security for him. If compensation for frustration started the drive in youth—and that theory cannot be proved—then ambition must have continued from habituation or, as the psychologists sometimes say, by "functional autonomy."

In the 1930s, when he was about sixty and the gifted children were being allowed to do some growing up, Terman was responsible for three other important books.

In 1937, with Maud A. Merrill and after ten years of work, he revised the Stanford Revision of the Binet-Simon scale of intelligence, the 1916 job, publishing the result under the title, *Measuring Intelligence*. They improved the tests greatly, brought the lower level down to the mental age of two years, adjusted the tests at three levels for superior adults, arranged the IQ ratings for adults (the highest IQ obtainable is 152), and made out two equivalent forms of the scale (124 items each) to permit retesting. Later this improved test became available for testing the offspring of the "gifted children" now grown up.

MASCULINITY AND FEMININITY; MARITAL HAPPINESS

While work on the Terman-and-Merrill revision of the Stanford scale was in progress, Terman and Catherine Cox Miles, with nine other assistants, were working on a scale for measuring masculinity

and femininity. The result was the publication in 1936 of *Sex and Personality: Studies in Masculinity and Femininity*. This project had also occupied about ten years of work. The scale, as it was finally established, consisted of two equivalent forms of 455 items each, which sought to elicit characteristic masculine and feminine interests and attitudes by way of word associations, associations for ink-blots, knowledge and information, emotional and ethical responses, interests, opinions, and position in the extrovert-introvert continuum. The scale ran from +200 at the masculine extreme to -200 at the feminine extreme. The mean male was found to rate at +52 and the mean female at -70. The means were far apart, but the spread between extreme cases was enormous; nevertheless, the overlap between the two sexes was small (only about 8%). About 1,500 subjects contributed to this study.

The results are characteristic of all of Terman's work, a mass of facts with no simple general theory emerging—a Stanley Hall kind of study, one might almost say. So you find that masculinity—as measured by the scale—increased in males up to the eleventh grade (+70 on the average) and then diminished steadily until old age (0 at age eighty), whereas femininity in females diminished up to the college sophomore level (-60) and then increased a little (to -90 at ages sixty to eighty). The least masculine male group is the old men of all occupations, and the least masculine male group at younger ages is the clergymen, who were still less feminine than the most masculine female group, the women college athletes. (There was one extreme group of women athletes as masculine as the clergymen.) And so on, as education, intelligence, occupation, and interests affect the score on the scale for each sex. There was a special study of homosexuality and another of delinquent girls.

One reviewer of this book remarked that the research “borders perilously on a laborious demonstration of the obvious,” but that remark is hardly fair. The quantification was new, the ability to say when there was overlap between the sexes and how much was new, the facts about age in the two sexes were largely new, and some of

the findings about homosexuality and about delinquent girls were unexpected. On the other hand, Terman found himself unable to come to a sure conclusion about the basic question as to whether psychological sex differences are due to nature or to nurture. He attacked Margaret Mead's case favoring environmentalism as unproven; he showed his prejudice for hereditarianism here as he had with intelligence; but the data were inappropriate for a decision.

Terman's third report of research in book form in the 1930s was his *Psychological Factors in Marital Happiness*, written with the assistance of four others, published in 1938. It was a survey of the hedonic state of 792 married couples and 109 divorced couples, 1,802 persons altogether. The data consisted in the results of personality tests and the responses to questions. The work led to the establishment of a scale of happiness, which had, as it turned out, a skew toward the more cheery extreme. On this scale it was possible to get scores of happiness and to relate them statistically to various supposed contributors to marital happiness.

What turned out was worth getting. Most of the supposed causes of marital happiness and unhappiness were not valid. Sexual relations mattered much less than had been anticipated. So did differences in age and in education between the spouses. The general conclusion could have been that happy persons make happy pairs. If one goes behind this truism, looking for causes, one can say that happy marriages depend most upon the superior happiness of the couples' parents, on the childhood happiness of the couples themselves, on the strength of attachment to mothers and fathers, and on the infrequency and mildness of childhood punishment.

This was all important information, but it must have fallen short of what Terman had hoped for. Description is not engineering, and you cannot in practice retrieve an unhappy marriage by finding happy parents for each of the unhappy couples. Nor is there yet apparent any good advice for the unhappy, as to whether they should stay unhappily unmarried or become unhappily married. There is this to say, though: An unhappy man or woman may have a happy

spouse. The hedonic gift is not dispensed in pairs, and the blame and credit assessed to marriage are perhaps more often a rationalization than a true statement of cause.

Terman, the avid constructor of mental tests, discovered, presumably to his initial surprise, that tests have a commercial value. The poor farm boy, the twelfth of fourteen children, who borrowed money to go to the Central Normal College, to Indiana University, and then to Clark University, who came away from Clark \$2,500 in debt and went to poorly paid teaching positions in San Bernardino and Los Angeles—twelve years of relative poverty with a wife and, later, two children—this boy, who became a professor at Stanford University, the author of the Stanford Revision of the Binet-Simon Tests of Intelligence, and, a little later, of the lucrative Stanford Achievement Test, discovered that the royalties on the tests were no inconsiderable sum when judged by academic standards. Terman's tastes were never extravagant. He continued to lead the life of a somewhat frail academic, but he could not have needed to worry about money after the Stanford Revision became the standard test of intelligence—one might almost say, had become the operational definition of intelligence—throughout the United States. He lived to see America become test-conscious. That he profited from the sale of the tests is incidental. We have already noted that his indefatigability was not diminished by his acquisition of an adequate income.

PERSPECTIVE

In 1956 it became the responsibility of the committee of the American Psychological Foundation to decide upon its second annual Gold Medal Award, the award for 1957, "to be given to an American psychologist with a distinguished and protracted history of scientific and scholarly accomplishment." The first Gold Medalist had been Robert S. Woodworth. The committee chose as the second Lewis Terman, but they were not quite in time. Terman died on December 21, 1956, and the award was not to be made until September 2,

1957, at the annual meeting of the American Psychological Association. The committee decided that posthumous awards are not desirable. They announced their intention, and honored Terman by finding for him no substitute, while the assembled members of the Association concurred by remaining silent for a brief interval.

Now let us try to obtain a perspective on the life of this leader of American psychology. Lewis Terman's outstanding characteristics were his drive and his love of learning. It is possible to interpret the drive as compensation for frustration, as original effort to transcend the limitations of a farmer's life, transformed by maturation into the personality pattern of an ambitious adult. That appeal to the environmentalistic explanation, however plausible, must remain speculative. Terman himself would have looked to heredity for his causation, yet there is no evidence that he was duplicating the pattern of his ancestors or his siblings, even though his son, with a very different youth, has also achieved academic eminence.

It is better, then, to take Terman as a fact, to content ourselves with describing how in fact he did escape from a farmer's life, not because he hated the life—he did not—but because he wanted learning more.

He was a farmer's boy, one of fourteen children. His father owned a few score of books, but there could be no luxuries in that home. He read the books, any book he could get hold of, and determined if possible to acquire an education. In that community the way out of farm life to education was by teaching school. So Lewis Terman took that route.

One sees in his unquenchable avidity for reading and schooling how strong his drive was. He would get through school, and still go on. His parents helped him at first when they could, but that was not much. At each new level he could see the one beyond. At Central Normal College he wanted Indiana University. There, in contact with Bryan, Lindley, and Bergström, he wanted Clark. At Clark the next pattern was set by Stanley Hall. His progress was, however, checked by the emergence of tuberculosis. What happened? Cer-

tainly this frustration intensified his determination. He accepted the geographical limitations of climate, but his ambition remained undiminished. San Bernardino was a step to Los Angeles. Always, when the tuberculosis checked him, he refused to surrender, took the minimal means for restoration, and was soon back on the job.

From Los Angeles he went to Stanford. Big ideas were occupying his mind. The revision of the Binet tests was his first big undertaking, and it was successful.

At first he had accepted his isolation as an educationalist, according to the pattern of Stanley Hall, but then he perceived the next level, the new world of scientific psychology, which he had known about at Clark and now was anxious to join. Teaching in the east in the summers, joining the American Psychological Association, being thrown by the psychological work of the First World War into contact with the other psychologists of the country, the growing success of the tests—all these things, plus the financial security that now at last came to him, gave him confidence in his own worth and importance. They gave him confidence, these things, but they did not release the tensions that drove him on.

For twenty years at Stanford as Executive Head of the Department of psychology, and for fifteen more as an active emeritus, he kept on enthusiastically with the lines of endeavor he had started. The second revision of the Binet tests, the Stanford Achievement Test, the measure of masculinity-femininity, the scale of marital happiness were efforts along the way, efforts that showed what tests could do and also how they might be found limited, but his main understanding was the *Genetic Studies of Genus*, his work with his “gifted children,” with whom he was in correspondence thirty-five years after the work began, an affectionate father figure, as he wanted to be and as many of the “children” regarded him. He kept writing to the “children,” and their spouses, asking about the “children’s” children and their lives; and they, accepting him as a father figure, replied and gave throughout the years many hours of their time in tests and questionnaires for both themselves and their children.

Terman himself liked to speculate on what would have happened to him if he had not gone to Indiana and met Lindley, if he had chosen Florida instead of California, if the death of Bergström had not led to his being called to Stanford. Such guessing is futile. There are no controls for history, and you cannot state general biographical laws. You can, however, describe what happened, and it is clear that Terman and the times were able to fit each other. Terman was inept with apparatus just when American psychology was becoming brass-instrument conscious, but he found Stanley Hall, an erudite entrepreneur of the mind who was not an apparatus man. Terman fitted the Hall pattern and all his life reflected something of what he learned in Hall's famous vitalizing seminar. Terman wanted to get to the top and did, the top of the particular mountain on which he was.

Lewis Terman was a friendly person. He liked people and wanted them to like him. To avoid a vigorous social life he pled habitually his physical frailty, for the tuberculosis had convinced him that he was not physically strong; yet he needed to be liked, he wanted love and affection from many, and his immediate colleagues and the others at a distance warmed to his eager friendliness. Like the gifted children the graduate students too thought of him as a father figure—especially those who attended his Stanley Hall-like seminar, held like Hall's own on a Monday evening, and those few who came beforehand to dinner, where Mrs. Terman always played the gracious hostess.

This need for friendly relations with his associates is consistent with Terman's liberal political philosophy. He believed intensely in freedom of teaching and freedom of thought, in the democratic process, and to some extent in the socialization of education and medicine. He wanted social justice, racial tolerance, equality of opportunity. He believed that the social sciences should set themselves to the task of civilizing man's impulses and emotions so as to make it possible for mankind "to live together in peace, justice and good

will.”¹² This is a philosophy wholly consistent with the reactions of the isolated farm boy who urgently sought success and friends among scientists and scholars.

Some persons, noting that he tended to believe in an hereditary intellectual élite, wonder how such an undemocratic view could be held by this tender-minded, sensitive, ambitious person, but the fact is that Terman thought of the intellectually élite as those who would save civilization for democracy. The gifted were given. You do not choose to have them, for there they are, whether you will or no. You can, however, choose to use them, to separate them from the crowd so that they may be trained to devote their special talents to benefit the crowd from which they have been taken.

That, then, is Lewis Terman, a sensitive man who wished to succeed and was strengthened by difficulty, as able a man as Stanley Hall’s seminar ever provided among the many able men that it produced, a widely read man who loved knowledge for its own sake, a clear and felicitous writer with a gift for the popular account that left the scientific values intact, a friendly chap determined to have affection and yet to keep pushing toward the top, the dean of America’s premathematical mental testers, a democratic liberal who believed that the intellectual élite, since they are a fact, must be used to promote a peaceful civilization in which new knowledge forever advances the human weal. In addition to his many contributions to modern scientific psychology, this practical demonstration in social philosophy may also in the future come to stand out as of great importance: Lewis Terman, a liberal in his thinking, showed, nevertheless, how democracy cannot avoid stratification as it is given by nature’s inevitable division of human material into different levels of ability.

¹² On Terman’s philosophy of life, see Hilgard’s biography, pp. 478f.

KEY TO ABBREVIATIONS

- Amer. J. Psychol.=American Journal of Psychology
Amer. Phil. Soc. Yearb.=American Philosophical Society Year Book
Amer. Psychologist=American Psychologist
Amer. Sociol. Rev.=American Sociological Review
Calif. Med.=California Medicine
Calif. Outlook=California Outlook
Charact. Pers.=Character and Personality
Commercial Law J.=Commercial Law Journal
Contemp. Psychol.=Contemporary Psychology
Dietetic Hyg. Gaz.=Dietetic and Hygienic Gazette
Educ. Forum=Educational Forum
Educ. Psychol. Monogr.=Educational Psychology Monographs
Educator-J.=Educator-Journal
Genet. Psychol. Monogr.=Genetic Psychology Monographs
Harp. Wkly.=Harper's Weekly
Indep. Educ.=Independent Education
Indiana Univ. Alumni Quart.=Indiana University Alumni Quarterly
J. Abnorm. Psychol.=Journal of Abnormal Psychology
J. Appl. Psychol.=Journal of Applied Psychology
J. Crim. Law Criminol.=Journal of Criminal Law and Criminology
J. Delin.=Journal of Delinquency
J. Educ. Psychol.=Journal of Educational Psychology
J. Educ. Res.=Journal of Educational Research
J. Educ. Sociol.=Journal of Educational Sociology
J. Except. Child.=Journal of Exceptional Children
J. Higher Educ.=Journal of Higher Education
J. Personnel Res.=Journal of Personnel Research
J. Phil.=Journal of Philosophy
J. Psycho-Asthen.=Journal of Psycho-Asthenics
J. Psychol.=Journal of Psychology
J. Soc. Psychol.=Journal of Social Psychology
J. Tchr. Educ.=Journal of Teacher Education
Los Angeles Bar Assn. Bull.=Los Angeles Bar Association Bulletin
Marriage Fam. Liv.=Marriage and Family Living
Mem. Nat. Acad. Sci.=Memoirs of the National Academy of Sciences
Mother's Mag.=Mother's Magazine
Nat. Educ. Assn.=National Education Association
Nat. Educ. Assn. J.=National Education Association Journal
Nat. Educ. Assn. Proc.=National Education Association Proceedings
Nat. Soc. Stud. Educ.=National Society for the Study of Education
New Eng. Mag.=New England Magazine

- New Repub. = New Republic
 N. Y. Times = New York Times
 N. Amer. Rev. = North American Review
 Occup. = Occupations
 Pac. Hist. Rev. = Pacific Historical Review
 Ped. Sem. = Pedagogical Seminary
 Pop. Sci. Mo. = Popular Science Monthly
 Psychol. Bull. = Psychological Bulletin
 Psychol. Clin. = Psychological Clinic
 Psychol. Monogr. = Psychological Monographs
 Psychol. Rev. = Psychological Review
 Riverside Educ. Monogr. = Riverside Educational Monographs
 S. F. Call = San Francisco Call
 Sch. Rev. = School Review
 Sch. Soc. = School and Society
 Sci. Amer. = Scientific American
 Sci. Mo. = Scientific Monthly
 Scribner's Mag. = Scribner's Magazine
 Sierra Educ. News = Sierra Educational News
 Soc. Sci. Res. Conf. = Social Science Research Conference
 Soc. Serv. Rev. = Social Service Review
 Sociol. Soc. Res. = Sociology and Social Research
 Stanford Illus. Rev. = Stanford Illustrated Review
 Train. Sch. Bull. = Training School Bulletin
 Univ. Calif. Chron. = University of California Chronicle
 Wash. Educ. J. = Washington Education Journal
 Yearb. Nat. Soc. Stud. Educ. = Yearbook of the National Society for the Study
 of Education
 Z. päd. Psychol. = Zeitschrift für pädagogische Psychologie

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HORATIO C WOOD, JR.

January 13, 1841–January 3, 1920

BY GEORGE B. ROTH

One should bear in mind that even if science and the scientific spirit were better understood and more widely spread, still we should not be too pleased with ourselves. Science, however necessary it may be, is not sufficient, and it will become more and more insufficient as it increases. There was a time when science sprang up as it were in the shadow of wisdom, but it has grown so exuberantly that wisdom is choked—and that is really frightening. If the same development continued without compensation, science would end in turning against humanity.—George Sarton. Foreword to Bernard Jaffe's *Men of Science in America* (1944).

AMONG THE PIONEERS in American science who rose to prominence in the latter part of the nineteenth century was a versatile Pennsylvanian: physician-teacher-scientist, Horatio C Wood.¹

¹ His name appears in the *Dictionary of American Biography* as Horatio Charles, and in the *Dictionary of American Medical Biography* (Kelly and Burrage) as Horatio Curtis.

His son, Dr. Horatio Charles Wood, Jr., in a personal communication to the author of this memoir, stated that his middle name was not Curtis or Charles, but simply C, without a period. Dr. Wood's father had wanted his name to be Horatio Curtis, but his mother wanted it to be Horatio Charles; so they compromised on Horatio C and this often confused the publishers of his writings.

The necrological list of Fellows appearing in the *Transactions of the College of Physicians of Philadelphia* for 1920, prints his name as Horatio C Wood.

Dr. Wood himself was not consistent in the use of his signature, having enrolled in the University of Pennsylvania as Horatio C Wood, Jr. This signature was used in correspondence with his commanding officers during his Civil War experiences in the Medical Corps of the U. S. Army and also in certain of his letters to the Secretary of the Smithsonian Institution. Later in life, he used the signature Horatio C Wood, Sr., apparently due to a desire to avoid confusing his name with that of his distinguished son, Horatio C(harles) Wood, Jr.



Horatio C. Wood Jr.

Dr. Wood was a Philadelphian of Quaker lineage, who attained distinction not only in the field of biological science but also in the field of medical education and in the science and art of medicine. He was outstanding not only at home but in foreign centers of learning.

Dr. Wood was born in Philadelphia on January 13, 1841. His father, Horatio Curtis Wood, was one of a family of seven, one brother being the distinguished teacher-physician and author, Dr. George Bacon Wood, who closely identified himself with the life and mental development of his nephew, the subject of these memoirs. This family was descended from Richard Wood, who came to Philadelphia in 1682 from Bristol, England, and is said to have come over with William Penn, presumably on the "Welcome." The family and descendants of Richard Wood were members of the Society of Friends, embracing this faith until the time of Dr. Wood and his descendants, some of whom, including Dr. Wood, became affiliated with other religious denominations.

Dr. Wood's mother was Elizabeth Head, daughter of John Bacon of Philadelphia.

On May 10, 1866, Dr. Wood married Elizabeth H. Longacre of Philadelphia, the daughter of the chief coiner of the U. S. Mint at Philadelphia.

The early educational training of Dr. Wood began at home. At the age of four years he was sent to the Friends boarding school at Westtown, where he was the smallest boy among more than two hundred pupils. At this school he obtained not only book knowledge, but also discipline and the capacity for enduring punishment without flinching. One of his biographers, a Philadelphian who in later life became a colleague in the scientific field states that Dr. Wood's father, coming from stern Puritan stock, believed heartily in vigorous discipline for his son. Apparently the discipline thus received from his father and his early schooling contributed to the development of a receptive and reasoning mind. As a boy he spent his summers in the country, which engendered a love for the beauties of

nature as well as its mysteries, also for hunting, which afforded him relaxation from his active professional life.

For his later education he was sent to the Friends Select School in Philadelphia, which Dr. Wood said saved him from the herd-teaching of a university education. While here an ex-college professor, Joseph Aldrich, who won his respect, coached him beyond most of his colleagues in Latin, aided him in acquiring an adequate knowledge of Greek and mathematics, and gave him special instruction in the construction and derivation of the English language. His interest in natural science took a more serious turn at the age of thirteen, during a visit to the Philadelphia Academy of Natural Sciences Museum. The Director of the Museum, Professor Joseph Leidy, found him near the display cases, crying because of his inability to examine and study the specimens by handling them personally and to read the books in the Academy library. Sensing the boy's true interest, Dr. Leidy arranged that the contents of any case as well as library books should be made available to him for study, thus starting the lad on the quest which led to many diverse investigations.

Dr. Wood enrolled as a student of medicine in the University of Pennsylvania in the session of 1859-1860, his preceptor being Dr. J. E. Rhoads. At that time, for a student to graduate, the medical school required attendance upon two complete courses of lectures (over a two-year period), a graduation thesis, and the attainment of the age of twenty-one, the latter requirement accounting for the fact that Dr. Wood did not receive his degree in medicine until 1862. His graduation thesis was entitled "Enteric Fever."

His first paper of a scientific nature was one on botany, which he published in 1860, at the age of nineteen, while a student in the University of Pennsylvania. This paper was a report to the Philadelphia Academy of Natural Sciences on the carboniferous flora of the United States of America. A second paper on this subject appeared in the same year, followed by a third in 1866. The interim between the first and the third paper was occupied largely in earning his degree in medicine, serving internships in several hospitals, and serving

as Acting Assistant Surgeon in the Northern Army during the latter part of the Civil War. His last assignment in the Army was at Fairfax Seminary General Hospital, which was located near Alexandria, Virginia. This was a front-line hospital, where he saw the horrors of war at close range. After his discharge in 1866 he returned to Philadelphia to engage again in the practice of medicine.

Upon his return to civil life he supplemented his practice of medicine by serving his alma mater as a private teacher or quiz master in the medical school. It was not long until he was given the chair of botany in the Auxiliary Faculty of Medicine in the University, a position he held until 1876. In addition, in 1873 he was elected Clinical Lecturer in nervous diseases in the Medical School proper, advancing in 1875 to the position of Clinical Professor.

Dr. Wood's career in medicine was primarily as a consultant, his early years being devoted chiefly to neurology and psychiatry. However, his interest in botany did not abate during the earlier years of his devotion to science. His botanical publications from 1860 to 1877 numbered fourteen, one of which, containing 270 pages with 19 colored and uncolored plates describing the fresh water algae of the United States, was published by the Smithsonian Institution of Washington, D. C. In his introduction to this monograph he said: "Although beset with difficulties at the outset, no branch of natural science offers more attractions than the fresh water algae. The enthusiasm of the student will soon be kindled by the variety and beauty of their forms and wonderful life processes. They have been found in healthy life, in the middle of an icicle and in the heated waters of a boiling spring."

In 1876 he was made Professor of *Materia Medica* and Pharmacy to which title was added, in the following year, that of General Therapeutics. Dr. Wood was retired in 1907 because of ill health, receiving the title of Emeritus Professor of Therapeutics.

The famous remark of Dr. Oliver Wendell Holmes that the early medical teachers "occupied not a chair but a settee" was truly exemplified in the appointments of Dr. Wood.

Throughout the entire period of his active life he maintained an investigative attitude of mind, the culmination of which was the development of a text on therapeutics, based not upon usage but upon evidence obtained primarily in the experimental laboratory. Of his seven books relating to the broad field of medicine, the one which brought him both national and international fame was his *Treatise on Therapeutics* which appeared in 1874 and was dedicated to his uncle, George B. Wood. The twelfth edition of this work was published in 1905 shortly before his retirement. This book was a revolt against empiricism or, as he harshly put it, "clinical experience." His aim was "to make the physiological action of remedies the principal point in discussion," and not secondary as had been the custom in preceding works on therapeutics. In his introduction to the first edition he states: "Experience is said to be the mother of wisdom, verily she has been in medicine rather a blind leader of the blind."

A further quotation from his introduction to the first edition reveals the breadth of his therapeutic horizon and indeed makes him one of the medical prophets of the nineteenth century. It read as follows: "A primary knowledge of the end to be accomplished and a secondary acquaintance with the instruments are a necessity for successful human effort; and until the sway of this law is acknowledged by physicians, medicine can never rise from the position of an empirical art to the dignity of applied science. Until within a comparatively recent period, it has been impossible to comply with this law. But, through the advances made by pathologists and by the students of the natural history of disease, we are fast learning the methods in which nature brings the body back to health."

The *Treatise* ushered in a new era in therapeutics and was adopted by most of the leading medical schools of this country. It was the principal American textbook in materia medica and therapeutics for about thirty years. One of this country's leading pharmacologists said that it had probably more influence than any other single book in this country in the origination of the idea of pharmacology as a distinct subject in the medical curriculum.

In Dr. Wood's earlier years, entomology as well as botany occupied his serious attention. From 1861 to 1869 he published over a dozen papers in this field of investigation. Most of these studies were reported in the *Proceedings of the Philadelphia Academy of Natural Sciences* or in the *Transactions of the American Philosophical Society*. His major studies in entomology were those on the North American myriapoda or centipedes. His monograph of 112 pages, entitled *The Myriapoda of North America*, was intended for and first accepted by the Smithsonian Institution, but while waiting its turn to be published the manuscript was destroyed by fire. The paper was subsequently rewritten but, as the funds for its publication by the Smithsonian had been depleted, it was, with the consent of the Institution, offered to and accepted by the American Philosophical Society for publication in the *Transactions*. This monograph on the myriapoda attracted the attention of Louis Agassiz who, upon returning from a collecting trip to Brazil, placed his entire collection of myriapoda at Dr. Wood's disposal for study and report.

In the field of medical education, especially in the latter part of the nineteenth century, Dr. Wood was aggressive in his reform attempts to bring the medical schools of the United States, and in particular the University of Pennsylvania, up to the standards set by a few of the American schools and those of the leading foreign schools. Writing upon this subject in *Lippincott's Magazine* for December, 1875, he said: "The methods of education pursued in this country, are at present singularly imperfect and the need of some control from without the profession is imperative."

Dr. Wood pointed out that at this time there were about 100 medical schools, whereas the natural demand should be for about a dozen. During 1874 in the German Empire with a population of about 42,000,000, there were 660 physicians licensed to practice medicine, whereas in the United States with a population of about 40,000,000, there were 3,000 graduates in medicine.

He urged that a thorough general education should precede the

study of medicine and that the technical and practical training in medicine should be in stages, namely, in botany and chemistry, then anatomy and physiology, to be followed by medicine and, finally, the clinical aspects of medicine, pursued both in theory and practice at the bedside.

He considered that a four-year course in medicine was the least time that should be required for graduation. This was a revolutionary idea, for at this time a large portion of students who had not received a solid year of training in medicine were entitled, with such limited training, to engage in the practice of medicine.

The interest and influence of Dr. Wood in the selection of members of the medical staff of which he was a member are shown by the following incident described by one of his colleagues. During the discussion of a successor to Dr. Alfred Stillé, who was retiring in 1884 from the Professorship of Medicine, Dr. Wood learned that Dr. William Osler, then of Montreal, was being considered for the vacancy. Although knowing of Dr. Osler's high attainments in his field, Dr. Wood, in the midst of a heavy schedule of duties, took time out to interview him in Montreal. On his return to Philadelphia, his report to his colleagues in Dr. Osler's behalf was so enthusiastic, and his presentation of Dr. Osler's qualifications so praiseworthy and convincing, that the result was the election of Dr. Osler to the Professorship of Medicine in the University of Pennsylvania.

Dr. Wood was a member of a number of scientific societies of high repute, among which were the American Physiological Society and the National Academy of Sciences. He was one of the five councilors of the former, chosen following the adoption of its constitution on December 30, 1887.

Howell, in his history of the first twenty-five years of the Society, wrote as follows: "The Society itself was fortunate in having among its original members, Mitchell (S. Weir), Wood (Horatio C), Welch (William), Osler (William), and Vaughan (V. C.), who were or soon became the acknowledged leaders of scientific medicine in this country." Howell further wrote: "Compared with similar organiza-

tions in science or medicine of the same period the Society set a relatively high standard of membership. The shibboleth demanded was research. There can be no doubt that the standard thus set and maintained has exerted a stimulating influence on research, not only in physiology but in other branches as well."

Dr. Wood was elected to membership in the National Academy of Sciences at the meeting held in Washington from April 15 to 18, 1879, in the rooms of the All Souls' Church. He was one of four to be so honored that year; the others were Cleveland Abbe, William G. Farlow, and Josiah Gibbs. Among those at this meeting who offered communications and were invited to take part in the scientific sessions of the Academy was Alexander Graham Bell, the title of his paper being "On Vowel Theories Considered in the Light of Recent Experiments with the Phonograph and Phonautograph." Bell was elected a member of the Academy in April, 1883.

Early in his career, Dr. Wood was offered professorships in the College of Physicians and Surgeons and in the Bellevue Hospital Medical School, both in New York City, but declined both offers. Later he was invited to become the Professor of Therapeutics in the Johns Hopkins University of Baltimore, and also to accept the Professorship of the Theory and Practice of Medicine in the Jefferson Medical College of Philadelphia, but again declined both, considering it best to be faithful to his alma mater. He was a firm believer in the influence of environment upon the success or failure of the individual, which may have led to his decisions to remain in Philadelphia. In this connection it should be recorded that his distinguished uncle, Dr. George B. Wood, an influential member of the medical faculty during and following Dr. Wood's student days, was one of the men whose advice Dr. Wood usually sought and respected highly.

Dr. Wood was an extensive contributor to medical literature. Three of his papers won prizes. The first (1869) was a special prize from the American Philosophical Society for his paper entitled "Research upon American Hemp" which has become a classic in the

literature of hemp (hashish). Two years later the Warren prize of the Massachusetts General Hospital was awarded to him for his "Experimental Researches in the Physiological Action of Amyl Nitrite," and in 1872 he received the Boylston prize for his paper on "Thermic Fever or Sunstroke."

Dr. Wood's studies on fever engaged his attention for many years, his interest perhaps having been fostered by his work on enteric fever, the subject of his thesis for graduation in medicine.

In 1875 he was invited to give the Toner Lecture, established in Washington, D. C., by Dr. Joseph M. Toner, a practicing physician in that city. This lectureship was placed in charge of a Board of Trustees, consisting of the Secretary of the Smithsonian Institution, the Surgeons General of the U. S. Army and the U. S. Navy, and the President of the Medical Society of the District of Columbia. The interest on the fund provided for this purpose was to be applied annually for an essay "relative to some new truth fully established by experiment and observation."

The invitation was accepted by Dr. Wood, whose essay was entitled "A Study of the Nature and Mechanism of Fever." It was published in the Miscellaneous Collections of the Smithsonian Institution in 1875.

This paper encouraged the Smithsonian Institution to assist Dr. Wood financially in further studies on fever, and in 1880 his later work in this field was reported in the Smithsonian's *Contributions to Knowledge* under the title of "Fever, a Study in Morbid and Normal Physiology."

Dr. Wood served as editor of the following medical journals: *New Remedies* (1870-1873), *Philadelphia Medical Times* (1873-1880), and *The Therapeutic Gazette* (1884-1890). He also edited the *U. S. Dispensatory* (still in existence) from 1883 to 1907, relinquishing the position shortly before his retirement from active work in the University of Pennsylvania School of Medicine.

When the English *Journal of Physiology* came into existence under the editorship of Michael Foster of Trinity College, Dr. Wood

was honored by being selected to be one of three Americans to act as co-editors, the other two being H. P. Bowditch of Boston and H. N. Martin of Baltimore. The first volume appeared in 1878-1879.

The honorary degree of Master of Arts was conferred upon Dr. Wood by Lafayette College in 1881 and he received its Doctor of Laws degree in 1883. He also received the honorary degree of Doctor of Laws from Yale University in 1889, and from the University of Pennsylvania in 1904. When the new Philadelphia General Hospital was completed, the members of its staff honored him by dedicating a ward to "Horatio C Wood, distinguished pharmacologist, therapist and clinician."

Dr. Wood was President of the College of Physicians of Philadelphia in 1902-1903, election to this office having occurred while he was on a trip to Egypt for his health. His first official address as President of the College of Physicians called attention to the need for an increase in the size of the building for the College if it were to prosper and its library be maintained. After four years of discussion, favorable decision to carry out his wishes in the matter was made, and a committee was appointed, among whom was S. Weir Mitchell, an adept in such matters. Sufficient funds, including a gift of \$100,000 from Mr. Andrew Carnegie, were soon obtained from members and friends.

Dr. Wood's connection with the U. S. Pharmacopoeia covered almost three decades, during two of which he served as President of the Convention, from 1890 to 1910. He considered the influence of the Pharmacopoeia upon medicine, through the establishment of standards for uniformity, especially for potent drugs, to be a most important one. This beneficial impact on medicine was fully appreciated by his colleagues and associates.

The Philadelphia *Public Ledger*, in its announcement of Dr. Wood's death, held that the crowning honor of his career came to him in 1902, when he was appointed to be a representative of the United States to the Brussels Conference, called by the Belgian Government for the unification of heroic remedies, the first en-

deavor to establish international standards for the definition and strength of the more important potent medicinal remedies in use throughout the world.

Dr. Wood was an ardent advocate of the use of animals as a preliminary step for ascertaining physiological functions and for determining the mode of action of drugs. He often appeared before state and federal committees to justify and explain his belief in their use. When he learned that certain members of the medical profession, who were inadequately informed, readily could be made to subscribe to the statements of opponents of animal experimentation, he answered one of his opponents as follows: "It is probable that no American believes in unrestricted vivisection; it is certain that 'unrestricted vivisection exists in most parts of the United States' is a falsehood. What the American physician contends for is that vivisection requires no special brand to be placed upon it; that the vivisectionist needs no other restriction than that placed upon all other members of the community by the general law. Every physiologist familiar with antivivisection literature knows that it is full of misstatements, so that this attempt to manufacture public opinion by the circulation of what should be called by the Saxon word 'lies' is concordant with the history of the past in warranting a strong suspicion that signatures to the document circulated . . . will not be honestly dealt with. I therefore suggest that the proper receptacle for the circulars is the waste-paper basket."

Dr. Wood's last public appearance was in June, 1904, the occasion being the formal opening of the new laboratories of physiology, pharmacology and pathology of the University of Pennsylvania. In 1906 his breakdown in health was so complete as to render him an invalid for his remaining years.

His last paper of scientific import was on the action of alcohol on the circulation, with Daniel M. Hoyt as the junior author. For this investigation he received financial aid from the National Academy of Sciences through the Bache Fund.

Dr. Wood's last public address was made in 1910, as retiring Presi-

dent of the United States Pharmacopoeial Convention. Because he was too ill to attend, the address was read by the secretary. Some excerpts follow: "Morituri Salutamus! From a sickbed the President of the United States Pharmacopoeial Convention of 1890-1900 salutes you and sends salutation to the men of the Convention of 1910, with a brief account of his stewardship during the last decade.

"Gentlemen of the Convention: In April, 1860, as a medical student, eighteen years of age, by finishing a paper on the carboniferous flora of the United States published in June by the Philadelphia Academy of Natural Sciences, I commenced my public scientific career; in April, 1910, with the writing of this address I close it. A half-century has passed, most of it spent in over-strenuous labor, so that the evil days spoken of by Solomon the Poet-King have come to me before the three-score and ten years have ended: and so I bid you farewell."

Dr. Wood's personal attributes were of the highest order, as may be gathered from the many tributes that were paid to him by his onetime students, colleagues, and other associates in their numerous memorial addresses honoring him. Many of these memorial addresses may be found in the *Transactions of the Philadelphia College of Physicians* for 1920, the year of his death.

Dr. F. X. Dercum described him as one "who possessed a unique and striking personality" who "made his presence instantly felt." "His face was strong and handsome, his voice full and round, his thoughts original. His language was vigorous and expressive . . . his personality, also, was one to inspire regard and affection. He had a group of devoted followers who from youth to maturity never faltered in their loyalty. . . . When we think of him, it will not only be of his scientific achievements, of his books and writings, but of his warm, his living, his inspiring personality."

Dr. Hobart A. Hare said of him: "He made his mark in medicine because he had visions of a coming dawn in medicine as a science, because he had the force and vigor, because he fought hard for the right whenever the opportunity arose . . . he filled young men with

enthusiasm and he taught them to think and by thinking to arrive at accurate deductions."

Dr. Wood's "Reminiscences," written after many years of suffering, gives one a clear insight into his guiding philosophy in life, which he describes as "Kindness and sympathy alike toward the highest-born and strongest, and toward the lowest and weakest."

In his early manhood he was very active in the Methodist church, in which he maintained an interest throughout life. His religious faith he summed up in these words: "The long bitter years of suffering have taught me to wish for speedy death, with the hope and faith that the sacrifice of Christ and the mercy of the Lord would bring me to Beatitude."

He had no fear of eternal punishment or death and could see no final fate in death other than "an escape from a life of suffering to an eternal sleep." "And so, striving during life to do the right as it is given us to see the right, may we go out with the cheery cry 'What of the night, brother, what of the night.'"

His death on January 3, 1920, in his eightieth year, resulted from pneumonia. He was buried in the city of his birth.

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KEY TO ABBREVIATIONS

- Am. J. Med. Sci.=American Journal of the Medical Sciences
 Am. J. Physiol.=American Journal of Physiology
 Am. J. Sci. Arts=American Journal of Science and Arts
 Boston Med. Surg. J.=Boston Medical and Surgical Journal
 J. Acad. Nat. Sci. Phila.=Journal of the Academy of Natural Sciences of Philadelphia.
 J. Am. Med. Assn.=Journal of the American Medical Association
 J. Nerv. Mental Dis.=Journal of Nervous and Mental Diseases
 J. Physiol.=Journal of Physiology
 Med. News=Medical News
 Phila. Med. J.=Philadelphia Medical Journal
 Phila. Med. Times=Philadelphia Medical Times
 Proc. Acad. Nat. Sci. Phila.=Proceedings of the Academy of Natural Sciences of Philadelphia
 Proc. Am. Phil. Soc.=Proceedings of the American Philosophical Society
 Smithsonian Contrib. to Knowledge=Smithsonian Contributions to Knowledge
 Smithsonian Misc. Coll.=Smithsonian Miscellaneous Collections
 Therap. Gaz.=Therapeutics Gazette
 Trans. Am. Phil. Soc.=Transactions of the American Philosophical Society
 Trans. Assn. Am. Physicians=Transactions of the Association of American Physicians
 Trans. Coll. Physicians Phila.=Transactions of the College of Physicians of Philadelphia
 Univ. Med. Mag. Phila.=University Medical Magazine of Philadelphia
 Univ. Pa. Med. Bull.=University of Pennsylvania Medical Bulletin

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